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LOGICAL ANALYSIS AND ONTOLOGICAL RECONSTRUCTION

TWO PROGRAMS IN THE ANALYTIC TRADITION

H. VISSER

LOGICAL ANALYSIS AND ONTOLOGICAL RECONSTRUCTION

TWO PROGRAMS IN THE ANALYTIC TRADITION

PROEFSCHRIFT

ter verkrijging van de graad van doctor
aan de Katholieke Universiteit Brabant,
op gezag van de rector magnificus,
prof. dr. R.A. de Moor,
in het openbaar te verdedigen
ten overstaan van een door het college
van decanen aangewezen commissie
in de aula van de Universiteit
op donderdag 2 april 1987
te 16.15 uur

door

HENDRIK VISSER

geboren te Leeuwarden

1987
Offsetdrukkerij Kanters B.V.,
Alblasserdam

Promotores: prof. S.J. Doorman M.Sc.
prof. dr. S. Silvers

die voldoet aan de coherentieconditie.

Idem.

XII

Er is geen volledige vetomachtvrije 5-plaatsige quasi-Condorcetfunctie die voldoet aan de coherentieconditie.

Idem.

XIII

Indien harmonische analyses van tonale composities gegeven worden in een terminologie waarin rekening wordt gehouden met toonhoogteverschillen die in een natuurlijke stemming optreden, dan kunnen zij in bepaalde gevallen experimenteel worden getoetst.

XIV

De analyse die Martin Vogel heeft gegeven van het eerste Tristan-akkoord kan experimenteel worden weerlegd.

Martin Vogel, Der Tristan-Akkord und die Krise der modernen Harmonielehre. Düsseldorf, Gesellschaft zur Förderung der systematischen Musikwissenschaft e. V., 1962

XV

Het revolutionaire karakter van de harmoniek in de eerste dertien maten van de inleiding van Tristan und Isolde van Richard Wagner berust hierop dat van een dominantseptiemakkoord niet alleen de grondtoon en van een mineursubdominatakkoord met toegevoegde sext niet alleen de kwint, maar ook de karakteristieke dissonant door een neventoon worden vervangen.

XVI

Aan het begin van de negenenveertigste maat voor het einde van de derde Leonoreouverture van Ludwig van Beethoven dient in alle stemmen *p* te staan.

Ouverture No. 3 to the Opera Leonore by Ludwig van Beethoven. Op. 72a. Edited by Max Unger. London, Eulenburg, n. d.

VI

NEEDHAMs mereologische theorie voor een tussenrelatie is incoherent.

Paul Needham, "Temporal intervals and temporal order". Logique et Analyse 25 (1982) 49-64

VII

De kennistheorie die LEHRER in 1974 formuleerde kan met een eenvoudig tegenvoorbeeld worden weerlegd. Zijn oplossing voor de loterijparadox komt daardoor op losse schroeven te staan.

Keith Lehrer, Knowledge. Oxford, Clarendon Press, 1974

VIII

De kennistheorie die NOZICK in 1981 formuleerde kan met een eenvoudig tegenvoorbeeld worden weerlegd. Zijn analyse van het skeptische argument komt daardoor op losse schroeven te staan.

Robert Nozick, Philosophical explanations. Cambridge, Mass., Harvard University Press, 1981

IX

In het licht van TUOMELA's handelingstheorie heeft McCARTHY zijn eigen "Advice taker" uit 1958 later niet meer overtroffen.

John McCarthy, "Programs with common sense". In: Mechanisation of thought processes. Volume I. London, Her Majesty's Stationary office, 1959.

Raimo Tuomela, Human action and its explanation. Dordrecht, D. Reidel, 1977.

X

Er is een domein van n-2 profielen waarvoor er geen vetomachtvrije n-plaatsige Condorcetfunctie bestaat die voldoet aan de onafhankelijkheidsconditie.

Henk Visser, "Adequate representations of Condorcet-profiles". Methodology and Science (to appear)

XI

Er is precies één volledige vetomachtvrije 5-plaatsige Condorcetfunctie

STELLINGEN

I

FREGE droeg in zijn Grundgesetze der Arithmetik kwalificaties van eigennamen over op objecten en functies. Nergens in zijn werk blijkt echter dat uit de logische structuur van een volzin conclusies kunnen worden getrokken over de structuur van de werkelijkheid.

Deze dissertatie, Hoofdstuk Twee

II

WITTGENSTEIN identificeerde in zijn "Logisch-Philosophische Abhandlung" logische eigenschappen van volzinnen van een taal met eigenschappen van feitelijke situaties. Voorzover de logische structuur van een volzin kan worden bepaald leert deze ons volgens WITTGENSTEIN tevens iets over de structuur van de werkelijkheid.

Deze dissertatie, Hoofdstuk Acht

III

HUME's causaliteitstheorie is een ongegronde generalisatie van het resultaat van een bestudering van botsende biljartballen.

An abstract of a Book lately Published; entitled A Treatise of Human Nature, etc. London, C. Borbet, 1740

IV

KANTs onderscheiding tussen Sinn und Bedeutung kan met recht klassiek worden genoemd.

V

HUSSERLs mereologische theorie kan in een eerste-orde-taal worden geformaliseerd.

Edmund Husserl, Logische Untersuchungen. Zweiter Theil: Untersuchungen zur Phänomenologie und Theorie der Erkenntnis. Halle a.d. Saale, Max Niemeyer, 1901

Ter nagedachtenis aan mijn vader

CONTENTS

ACKNOWLEDGEMENTS

This dissertation was written during the ten years that I had an appointment at the Erasmus University Rotterdam to teach and investigate the "analytic method in philosophy".

I am deeply indebted to prof. S.J. Doorman who, despite his full schedule, acted as my advisor. It was Joop Doorman who urged me to continue this work.

During my Rotterdam period, I presented my views before different audiences, such as the Philosophy Department of Delft University of which Roger Cooke is a distinguished member, the Amsterdam Logic Colloquium of Dick de Jongh, and the New York Logic Colloquium of Elliott Mendelson. From these discussions, I learned that the analysis of the last fifty years of logical philosophy is better left to the future. This period is dealt with only in the Epilogue.

More recently, in my new position at Tilburg University, I have had the opportunity to consult with prof. Stuart Silvers, both on matters of content and English expression.

There are many friends who in one way or another have helped me to complete this book. Carien de Ruiter did not only an excellent job of typing many pages of text, but also performed the difficult editorial task of preparing the final version. Philibert Schogt revised the English and made helpful suggestions for improving the style. Nicole Oomen patiently put the text on the data text terminal; José van Grimbergen-Kolsteren typed the formulas of two earlier versions with great care; Marie José Vlaanderen supplied reliable assistance in regard to the bibliography. I thank them all.

INTRODUCTION

The term 'philosophical logic' is normally used for all those disciplines which investigate the "logic" underlying fragments of natural languages, by evaluating the validity of reasoning conducted in such fragments. According to this standard use of the term, philosophical logic comprises serious subjects such as tense logic, deontic logic and epistemic logic (cf. Rescher 1968a, p. 31). The qualification "philosophical" has nothing to do with the sort of philosophy which Bergmann (1964a, p. 340) once described as a mixture of ignorance, anti-science and mediocre literature.

However, the way in which philosophical logic has been practiced was qualified by some as philosophical in the sense that ontological assumptions were made in the characterizations of the logic of fragments of a natural language. Some of them even rejected, e.g. possible world semantics because they did not accept its "inventory of dubious entities" (Martin 1975b, p. 155). Others defended such an approach because of their belief in the existence of entities called "ways things could have been", "possible worlds" for short (Lewis 1973a, p. 84). Philosophical logic thus seemed to be philosophical in a deep traditional sense, the assumption being that ontology - reflections on what there is - was the hard core of the classical philosophical enterprise.

There is an alternative opinion, pleasing to those philosophical logicians who don't feel the depth of philosophy in that sense at all, but restrict the "philosophical" task of philosophical logic to the solution of logical puzzles such as "the Sea-battle", "the Good Samaritan" and "the unexpected examination". In a contribution to a seminar on ontology, Van Fraassen offered the following three theses concerning philosophical logic, the first of which dismisses the ontological turn (the italics are mine) (1973a, p. 119):

(T1) Philosophical logic can be done in such a way as to impute <u>no</u>

ontological commitment through any use of language.

(T2) Orthodox logic is inadequate to the analysis of natural language because there are important semantic properties and relations that cannot be characterized in orthodox semantic terms.

(T3) In the special situation of philosophical logic, correct methodology requires innovations and complications to occur on the side of the formal apparatus.

I believe that Gottlob Frege, the great innovator in the field of philosophical logic of the past, made precisely such assumptions in developing logical theories. Contrary to a popular interpretation of Frege's logical work, I consider Frege a philosophical logician who, as early as 1879, demonstrated by his logical work eo ipso that the three theses could be maintained. Thus, the fact that some writers began to think that it is impossible to do philosophical logic without ontological commitment cannot be explained by referring to the conceptual beginnings of the modern development of philosophical logic with Frege. Another source must be indicated.

First I defend my view on Frege's methodology of philosophical logic. I do this in Part One. My argumentation will consist of three parts, each of which corresponds to a different period in Frege's career as a philosophical logician: (1) from Begriffsschrift (1879a) to "Ueber formale Theorien der Arithmetik" (1885b), (2) from Function und Begriff (1891a) to the second volume of Grundgesetze der Arithmetik (1903a), and (3) from Russell's first letter to Frege to the Postscript of Grundgesetze. In this way, I can concentrate directly upon Frege's activities in the field of philosophical logic.

Sluga (1976a) stresses the historical situation in which Frege found himself. He argues that to approach Frege's thought from an ontological view is to approach it completely unhistorically. I agree with this, but am not completely convinced by Sluga's own historical approach, which results in the conclusion that for Frege, all considerations about logic are epistemological. I comment on this, by adducing some historical arguments.

Second, as already noted, between Frege and Van Fraassen there have been philosophical logicians who disagreed with at least one of the three theses, especially the first one. According to these philosophical logicians, logical work involves ontological questions as to the acceptability of the logical distinctions which are considered to be sufficient for the characterization of validity. This provides me with the following problem:

> What reasons did philosophical logicians have for believing that they were doing ontology when they were engaged in philosophical logic, so that that they rejected certain formal apparatuses and tried to force semantical phenomena into a Procrustian bed of their own logical theory?

I shall argue that those philosophical logicians confounded two different disciplines in the logical-analytic tradition, which I have characterized (cf. Visser 1981a) as _logical analysis of language_ and _logical reconstruction of world views_. Logical analysis of language is a part of philosophical logic, whereas logical reconstruction of world views is a discipline which investigates (possible) ways of conceiving the nature of the existing world by formalizing and axiomatizing these conceptions (Whitehead 1906a, p. 11). Logical reconstruction of world views comprises not only well-known results of Whitehead (in "On mathematical concepts of the material world"), Russell and Wiener (on time), Carnap (in _Der logische Aufbau der Welt_) and Goodman (in _The structure of appearance_), but also Woodger's biological axiom-system. In short, it concerns formal representations of knowledge, even of non-scientific ("common") knowledge, as can be seen from publications of Russell and Bergmann. It can have a traditional philosophical aspect in so far as logical reconstructionists see ontological or epistemological positions reflected in the construction of their axiomatic systems and choose among these systems on ontological or epistemological grounds. (Epistemology - reflections on what can be known - may not be the core of the classical philosophical enterprise, but it certainly has been the major concern of philosophers since Descartes, so by "philosophical" arguments and positions, I shall henceforth understand ontological or epistemological arguments and positions.) It is this aspect of the logical reconstructions of world views which in my view has been imposed on logical analysis of language, as if it were a logical reconstruction of world views. This resulted in the above-mentioned view of philosophical logicians that they made ontological assumptions in their characterizations of the logic of fragments of a natural language.

Support for this opinion can be found in ... Van Fraassen's book The scientific image (1980a), in which he posited two theses concerning the confusion of analytical disciplines (1980a, p. 196):

(T4) Certain issues in philosophy of science have been misconstrued as issues in philosophy of logic and language.

(T5) Important philosophical problems concerning language have been misconstrued as relating to the content of science and the structure of the world.

I omit discussion of (T4), since I am not concerned with philosophy of science here. That is to say, I shall not deal with "logical reconstructions" of concepts which are used in order to talk about scientific results or procedures. But (T5) seems to hit the nail on the head. Logical analysis of language deals with problems concerning language, and logical reconstruction of world views with problems which relate to the content of science and the structure of the world. Assuming that in logical analysis of language one is doing ontology, misconstrues problems of philosophical logic as problems of logical reconstructionist philosophy.

Van Fraassen (o.c., p. 196) wrote that to substantiate the view which is expressed in the last two theses requires a theory of language as well as a theory of science. Similarly, my thesis about the confusion of logical analysis of language with logical reconstruction of world views requires an exposition of these two disciplines. I have chosen a historical approach in the sense that I shall try to show how these disciplines made their appearance and were developed in the earlier stages of the analytic tradition. They predate the problems of their (supposed) relation that arose in Wittgenstein's "Logisch-Philosophische Abhandlung". Following Part One on Frege's methodology of logical analysis of language, (the methodology of) logical reconstruction of world views will be discussed in Part Two. Part Three will deal with some of the historical origins of the above-mentioned confusion and with "philosophical logic" as it was conceived by Russell and Wittgenstein in their respective philosophies of logical atomism. In the Epilogue, it will be argued that Russell's conception of "philosophical analysis" has survived in modern analytical literature and that the above-mentioned confusion did not disappear after Van Fraassen's contribution to the New York Symposium on ontology.

FREGE

PART ONE

FREGE

Introduction to Part One

According to Van Fraassen (1973a), the ultimate aim of philosophical logic is to give an exact description of the semantical structure of fragments of language by mathematical means. We can also say that a philosophical logician aims at gradually better (mathematical) characterizations of the notions of a true sentence (under a given interpretation) and of entailment (cf. Montague 1970a) [1]. Formulated in either way, analyses in terms of "propositions", "facts", "events", "possible individuals", "possible worlds", "pieces of information", "data sets" and whatever else there is, are not the final goal of philosophical logic. At most, these analyses have only a provisional, heuristic, didactic or illuminating function and this amounts to saying that they have no philosophical, i.e. ontological or epistemological significance. They can be seen as informal preparations for (or as informal elucidations of) formal expressions of mathematical descriptions - dependent on the stage at which they are performed. The chosen formulations are metaphorical; the question whether truth-values "really exist", taken literally, is meaningless. Thus, when David Lewis (1973a), in his defense of possible world semantics, says that he believes in the existence of entities that might be called "ways things could have been", this has nothing to do with the question of the adequacy of possible world semantics. Mutatis mutandis, the same holds for Richard Martin when he says of propositions that "at best they seem obscure, unanalyzed, abstract entities, and hence not suitable as a tool of philosophic analysis".

In sharp contrast to these authors, Van Fraassen confesses that he can't "even imagine wondering seriously whether there are sets or propositions". He gave the following account of logical analysis of language:

> I <u>use</u> or <u>engage</u> in mathematical discourse to describe other forms of discourse. (...) using mathematical discourse we can or ought, to provide rational reconstructions of other forms of discourse: modal, epistemic, deontic, discourse about possibles, about implications, about facts, and much more. In the explication of modality, for instance, I distinguish three moments: a language game with modal qualifiers, an account thereof in pictorial language (about possibles of possible worlds), and a formal reconstruction in mathematical language. The pictorial account is a guide to the formal account, but the sole object of semantic analysis is to provide a precise representation of the structure of the language game.

From this position, Van Fraassen obtains the three theses cited in the Introduction. As an example of semantic properties and relations which cannot be characterized in orthodox semantic terms, he chooses a language game with predicate modifiers. This contains statements such as 'Although swimming fast, John crossed slowly'. In order to represent the semantic structure of this language game Van Fraassen enlarges the traditional apparatus of "extensions" and "intensions" with so-called "comprehensions". I shall not go into the details of his analysis, but I would like to draw attention to a comparable situation in the history of philosophical logic: Frege's supplementation of his own apparatus with extensions of concepts. In comparing Frege's situation to Van Fraassen's, I have in mind Frege's <u>methodology</u> of logical analysis, which also satisfies the first three theses, differences in special aims notwithstanding. In this part, I shall concentrate on Frege's methodology that can be discerned from his writings between 1879 and 1906, the time that he was actively concerned with the foundations of logic and mathematics. I distinguish three periods, the early period of <u>Begriffsschrift</u>, a second period in which Frege worked with a revised logical system, culminating in the first volume of <u>Grundgesetze der Arithmetik</u>, and a third period in which Frege tried to repair his second system because of its inconsistency shown by Russell.

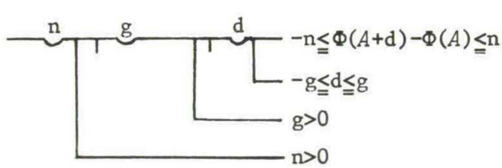
Frege formulated his general methodological position already in the first period, when he defended his logical theory against attacks from adherents of Boolean theories. As this general position seems not to have changed in the course of time, I have decided to discuss Kluge's (1980a) interpretation that Frege's logical distinctions amounted to "metaphysical" or ontological distinctions in my treatment of the first period. A similar position, taken by Bell (1979a) and especially directed towards Frege's second system, will be discussed in connection with a treatment of Frege's procedures in his second period. Finally, I shall reinforce my claim that Frege's logical distinctions did not lead him into an ontological direction, by referring to Frege's own discussion of possible "ways out" of Russell's contradiction in the postscript to the second volume of Grundgesetze der Arithmetik.

CHAPTER ONE

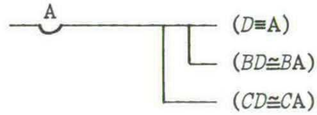
FREGE'S BEGRIFFSSCHRIFT

Introduction

It may seem preposterous to attribute to Frege a Van Fraassen-like methodology, but I believe that this can be defended on the basis of two papers from Frege's Nachlass. It appears that Frege was very keen on defending his first logical system. This system, including a "Begriffsschrift" as its syntax, can be considered an instrument for logical analysis in the sense that it allows a formal representation of at least partially informal sentences which occur in ordinary and in scientific discourse. What this amounts to can already be seen in Frege's treatment of some mathematical statements. Here he takes advantage of the circumstance that the mathematical language was already formal to a large extent. For example, in order to represent the well-known epsilon-delta-definition of continuity - of a real valued function of one variable for a given argument - Frege (1969a, p. 26-27) had only to add a few logical signs to the extant mathematical symbols [2]:



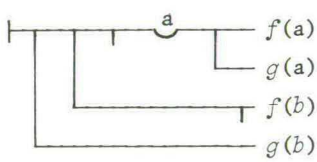
The same logical signs, the generality sign with accompanying variable, the negation sign and the sign for the material implication, together with the sign for an assertible content [3], can be used in a formal representation of a simple geometrical statement, such as that the point D lies in the straight line determined by the points B and C. Thanks to the fact that geometry already has a sign for the relation of congruence, one has only to add an identity sign:



In addition, Frege showed that formal representations of informal statements of a natural language can be given, provided that appropriate signs for what Frege called "particular contents" are chosen. We encounter an elaborate example in the following German statement, which, by the way, is very similar to a reasoning given by Zimmermann (1860a) in his Philosophische Propaedeutik [4]:

> 'wenn dieser Strauss ein Vogel ist und nicht fliegen kann, so ist daraus zu schliessen dass einige Vögel nicht fliegen können'.

Formula 59 of Begriffsschrift gives the following representation (1879a, p. 51):



The bold vertical sign in front of the formula shows that what is expressed is always the case, no matter what 'b', 'g' and 'f' signify. Indeed Frege succeeded, in modern terms, in axiomatically characterizing the logic which underlies fragments of the German language of which the above-mentioned statement belongs is an example. In other words, he provided a precise representation of the structure of a language game with simple connectives and quantifiers. Four years earlier, Drobisch (1875a, p. 5) had proclaimed that such a purely synthetic construction of logic following the example of mathematics was not suitable or hardly feasible.

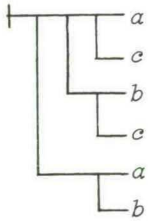
This is very remarkable in the light of Frege's repeated confession that he strived toward a "lingua characterica" which had "to paint the thoughts, not the words" as Leibniz said (Frege 1969a, p. 14). Such a characteristica universalis would require "a notation of thought contents and relations" (cf. Verburg 1951a, p. 270), in which both the "conceptual content" of thoughts, and the forms of their connections are represented with special signs (cf. Frege 1969a, p. 14). This is no idle talk: for Frege, two sentences have the same conceptual content if and only if all consequences which can be drawn from the first sentence in connection with certain other judgments, always follow also from the second sentence in connection with these judgments and conversely. For example: 'Bei Plataeae siegten die Griechen über die Perser' and 'bei Plataeae wurden die Perser von den Griechen besiegt' (1879, p. 3). With this example, Frege also argued that natural languages include phenomena which result only from the interaction of the speaker and the hearer. He noticed that natural languages leave much to be guessed: first, because the composition of words only imperfectly corresponds with the structure of the concepts, and second, because logical relations are often not expressed at all. In short, both on a syntactical and a semantical level, natural languages fail to express what is needed for correct deductions, whereas they give more than is needed for this on a pragmatical level. The logical structure of a language game is not precisely reflected in the linguistic medium.

These findings were not at all new: Riehl (1877a, p. 53) had stressed the same points in his discussion of the contemporary English logic [5]. But he did not even try to show that Boole's logic had overcome the imperfections of language, merely stating that Boole started "from the language of science, the mathematical sign language, in which the processes of thought find expression precisely and clearly". On this point Frege was much more critical. He remarked that both Boole's formula language and the arithmetical symbolic language solved only a part of the task of a lingua characterica or "conceptual notation" - Begriffsschrift - . It is true that arithmetic was already in the possession of a notation for many thought contents, but a notation for thought relations had still to be added, as Frege showed in the first given example. He succeeded in formally representing the concept of continuity where traditional arithmetic had to appeal to the word language (cf. Frege 1969a, p. 14). Mutatis mutandis, the same holds for geometry, and the second example shows that Frege was successful here too. But in the third example, Frege confined himself to the representation of sentences of the German language with the help of arbitrarily chosen letter combinations. Apparently, the application of Frege's notation to sentences and reasonings of a natural language did not have to wait until all complex concepts were analyzed in their ultimate constituents. Frege (1882a, p. 55) was aware of this when he outlined what he demanded of a true conceptual notation:

> Sie muss für die logischen Beziehungen einfache Ausdrucksweisen haben, die, an Zahl auf das Nothwendige beschränkt, leicht und sicher zu beherrschen sind. Diese Formen müssen geeignet seyn, sich mit einem Inhalte auf das Innigste zu verbinden. Dabei muss solche Kürze erstrebt werden, dass die zweifache Ausdehnung der Schreibfläche für die Uebersichtlichkeit der Darstellung gut ausgenutzt werden kann. Die Zeichen von inhaltlicher Bedeutung sind weniger wesentlich. Wenn die allgemeinen Formen einmal vorhanden sind, können jene leicht nach Bedürfniss geschaffen werden. Wenn es nicht gelingt, oder nich nöthig erscheint, einen Begriff in seine letzten Bestandtheile zu zerlegen, kann man sich mit vorläufigen Zeichen begnügen.

Before the ideal of a characteristica universalis has been reached, results can be gained also outside mathematics. It is true that Frege originally attempted to supplement the formula language of mathematics with symbols for the logical relations in order to first produce a lingua characterica or "Begriffsschrift" for the field of mathematics (1882a, p. 55). But Frege was sure that his notation was not limited to mathematics, because "the logical relations recur everywhere, and the symbols for the particular contents can be chosen in such a way that they fit into the framework of the conceptual notation" (1882a, p. 56). This means that Frege's system was also useful as an instrument for the logical analysis of fragments of a natural language, even though a complete pasigraphy, characteristic, or general conceptual notation did not yet exist, as Schroeder (1880a, p. 81) had written.

This reviewer of Frege's Begriffsschrift had remarked that its title promised too much: instead of leaning toward a universal characteristic, the booklet went in the direction of Leibniz's calculus ratiocinator (1880a, p. 82). Schroeder would have called Frege's booklet "very creditable" if a large part of what it pursued had not already been accomplished from another side, in a doubtlessly more adequate way. He demonstrated this by comparing the way in which Frege represented his results with the Boolean calculus. Take for instance Frege's important theorem 5:



This has an immediate application in the German language; for example it shows that the following judgment is true: 'wenn der Satz gilt, dass E magnetisch wird, sobald durch D ein galvanischer Strom fliesst; wenn ferner der Satz gilt, dass ein galvanischer Strom durch D fliesst, sobald T niedergedrückt wird: so wird E magnetisch, wenn T niedergedrückt wird'.
Now Schroeder represented the same theorem with the help of conjunctions, disjunctions and negations as [6]:

$$a_1c(b + c_1)(a + b_1) = 0$$

and said that the formula immediately becomes evident when it is multiplied out. His general view was that Frege's conceptual notation was devoted to the establishment of a formula language, which essentially coincided with Boole's representation of judgments and the calculation with them - with the exception of what was said on p. 15-22 of Begriffsschrift about "the function" and "generality" and the

"Supplement" beginning on p. 55 (Schroeder 1880a, p. 83). Schroeder admitted that the Boolean theory required further development but failed to see that Frege had achieved more than Cayley and Peirce. He did not see that Frege's syntactical and (informal) semantical characterization of full propositional logic was more than a method of "either directly or indirectly enumerating and summarizing which cases remain if one eliminates from all imaginable ones those excluded by the premisses" (1880a, p. 91; translation by Bynum 1972a, p. 229).

Schroeder disagreed with the writer of the Jena review - Lasswitz - that the Boolean theory "rested on an inadmissible conception of concepts" or on "doubtful presuppositions anyhow" - at least so long as the proof of this had not been delivered.

In a reaction to Schroeder's review - "Boole's rechnende Logik und die Begriffsschrift" - Frege (1969a, p. 39) produced one argument of such a proof:

> Ich glaube, dass fast alle Fehler, die beim Schliessen gemacht werden, in der Unvollkommenheit der Begriffe ihren Grund haben. Boole setzt logische vollkommene Begriffe als fertig und damit den schwierigsten Teil der Arbeit als getan voraus und kann dann aus gegebenen Voraussetzungen seine Folgerungen durch ein mechanisches Rechnungsverfahren ziehen.

In other words, Boole's logic is only a calculus ratiocinator but Frege's conceptual notation has the wider aim of analyzing concepts. Due to the imperfections of language, the structure of the concepts cannot be read off immediately from the words; supposing that it can is either dubious or leads to a very limited logic for concepts. Indeed, Boole's logic of concepts is nothing more than a simple class algebra. Moreover, Boole's letter signs never signify individual things, but always extensions of concepts, whereas Frege insisted that one has to distinguish between "concept" and "object", even in the case that only a single object "falls under" the concept; for one can have two different concepts under which the same object falls. This way of speaking is characteristic with concepts (1969a, p. 20):

> Bei einem Begriffe sind immer die Fragen möglich, ob etwas und was etwa unter ihn fälle, Fragen, die beim Einzeldinge sinnlos sind.

Boole's system indeed failed to represent most of the possible consequences which can be drawn from a simple arithmetical statement like '2 ist eine 4te Wurzel aus 16', such as 'Es gibt zwei Zahlen wovon die eine 4te Wurzel der anderen ist', let alone 'Es gibt zwei Zahlen so dass die eine eine natürliche Wurzel der anderen ist' or 'Es gibt drei Zahlen so dass die erste ein Logarithmus von der zweiten bei der dritte als Basis ist'. Similarly, Boole could not draw the consequences from statements about, e.g. prime numbers, having to do with the content of the concepts involved. Frege, on the other hand, had analyzed several arithmetical concepts - among them the concept of prime number - in terms of the concept of succession in a series (determined by a given function). As a result, he could easily infer from the judgment that 13 is a prime number that 13 is not divisible by 4, given that 4 is a positive whole number different from 1 and 13 (cf. Frege 1969a, p. 24-25).

One does not need the technical details to see that Frege's criticism of Boole's presupposition that "logically perfect concepts" are given, did not rest on any philosophical foundation whatsoever. It is true that one can represent concept formations with the help of the Boolean signs but, as Frege pointed out, the possibilities are very limited. (It goes without saying that Boole could not analyze the concept of continuity of a real valued function, let alone of the concept of succession in a series, which Frege traces back to his logical vocabulary in <u>Begriffsschrift</u> - via his well known definition of 'der Umstand, dass die Eigenschaft F sich in der f-Reihe vererbt'.)

Frege's methodological criteria

In his overall comparison of his logical system with the Boolean logic, Frege also did not resort to philosophical arguments. He had his criteria, of course, but they were not of an ontological or epistemological character. This appears from the following six conclusions which Frege reached in the same essay.

The first and second concern the scope:

> Meine Begriffsschrift hat ein weiteres Ziel als die Boolesche Logik, indem sie in Verbindung mit arithmetischen und geometrischen Zeichen die Darstellung eines Inhaltes ermöglichen will.
>
> Auch auf dem Gebiete des vom Inhalte absehenden rein Logischen beherrscht sie dank dem Allgemeinheitszeichen ein etwas weiteres Gebiet als die Boolesche Formelsprache.

The third concerns coherence:

> Sie vermeidet das Zerfallen der Booleschen Logik in zwei Teile (primary and secondary propositions) dadurch, dass sie das Urteilen als dem Bilden der Begriffe vorausgehend auffasst.

The fourth concerns significance:

> Sie ist im Stande, Begriffsbildungen darzustellen, wie sie die Wissenschaft braucht, im Gegensatz zu den verhältnismässig unfruchtbaren multiplicativen und additiven Verbindungen Booles.

The fifth concerns economy:

> Sie bedarf für die logischen Beziehungen weniger Urzeichen und daher auch weniger Urgesetze.

There is also a question of <u>simplicity</u> (with respect to a task):

> Man kann mit ihr Aufgaben lösen von der Art der Booleschen, und zwar mit weniger algorithmischen Vorbereitungen. Auf diesen Punkt lege ich am wenigsten Gewicht, da solche Aufgaben selten oder nie in der Wissenschaft vorkommen werden.

On different occasions, Frege resorted to one or another of these criteria. Let us look at his choice of the material implication as a primitive binary connective [7]. In <u>Begriffsschrift</u>, Frege preferred the material implication to the conjunction, because deductions seemed to be expressed more simply that way. In "Ueber den Zweck der Begriffsschrift", Frege defended his choice by stressing the importance of the relation embodied by his symbol for the representation of hypothetical judgments. These have the form "wenn etwas die Eigenschaft X hat, so hat es auch die Eigenschaft P" in the German language and that is, according to Frege (1883a, p. 6), "the form for all laws of nature, and for all causal connections in general". ("Ist doch das hypothetische Urtheil die Form für alle Naturgesetze, für alle ursächlichen Zusammenhänge überhaupt.") (Isn't this an ontological criterion of adequacy? I don't believe so: only a wrong translation of the German text - which has 'für' - can be misleading. But I am prepared to admit that Frege went too far here in giving these "a priori insights into the possible forms of scientific propositions".) It would have been sufficient to point to some laws which needed the material implication for their formulation (1891a, p. 1):

> Ein wissenschaftlicher Ausdruck erscheint da zuerst in seiner ausgeprägten Bedeutung, wo man seiner zum Aussprechen einer Gesetzmässigkeit bedarf.

The same appeal to significance was given in "Booles logische Formelsprache und meine Begriffsschrift" where Frege (1969a, p. 58-59) said that the outstanding importance of hypothetical judgments had induced him to give the sign

the meaning of the denial of the case "not A and B". In "Booles rechnende Logik und die Begriffsschrift", Frege handled the matter differently. There, he justifies his choice of the material implication with an appeal to greater economy. Frege was convinced that he needed fewer axioms than Boole because the meaning of his basic binary connective sign was simpler than Boole's addition sign. The latter connects two assertible contents A and B in the sense that two possibilities, "A and B" and "not A and not B" are excluded. (We would say that 'A *sive* B' is false if and only if A and B are both true or both false.) Boole's multiplication sign "says even more", because it negates three possibilities and leaves no further choice (Frege 1969a, p. 40-41). "Now when it is possible to come out with one single symbol - that negates one of the four cases - one should do it: for the fewer primitive signs one introduces, the fewer axioms one needs, the easier the command of the formulas becomes" (Frege 1969a, p. 57) [8]. Frege also appealed to "the essence of explanation" which in his opinion, consisted in the explanation's governing of a large or perhaps immense multitude by one or few sentences (1969a, p. 40). The matter is not limited to the primitives; when Frege limited himself to *modus ponens* as a single deduction rule he adhered to the requirement of "perspicuity" - *Uebersichtlichkeit* - (1879a, p. 9) or of "scientific economy" - *wissenschaftliche Sparsamkeit* - (1893a, p. 26). This is explicitly no psychological issue, but settles a form question in the sense of the greatest suitability. (It confirmed Sigwart's statement that all kinds of deduction of simple assertions can be reduced to the modus ponens alone, if the modus tollens can be reduced to the modus ponens (Sigwart 1873, p. 374); Frege (1879, p. 43) showed this possibility by introducing formula 28 of *Begriffsschrift*.)

One of the most important innovations of Frege's conceptual notation is, of course, his notation for generality with the help of variables and a universal quantifier for binding them. With this, Frege's system exceeds Boole's not only in scope, but also in coherence: an organic connection - organischer Zusammenhang - between the primary and the secondary propositions is established by it, in place of the Boolean artificiality - Künstelei - (1883, p. 9). Boole's system consisted of two parts, an algebra of classes and an algebra of propositions; the latter part could be reduced to the former with the help of a special interpretation. For example, the judgment "if $x = 2$, then $x^2 = 4$" was interpreted by Boole as "all members of the class of times in which $x = 2$, are also members of the class of times in which $x^2 = 4$". The example is Frege's, who raised two principal objections against this conception. First, it has the disadvantage that time becomes involved where it doesn't belong (1883a, p. 4). In order to understand this, we have to realize that Boole took his own interpretation very seriously; he explained that the language of common life sanctions the view that there is an essential connection between secondary propositions and the notion of time. "Thus we limit the application of a primary proposition by the word 'some', but that of a secondary proposition by the word 'sometimes'. To say 'Sometimes injustice triumphs' is equivalent to asserting that there are times in which the proposition 'Injustice now triumphs' is a true proposition" (Boole 1854a, p. 163). But then Boole's theory of secondary propositions is either very limited in applicability indeed, as Boole admitted with a reference to "eternal truths", or the idea of introducing times is only an "auxiliary idea" - Hilfsvorstellung - which is not very to the point, as Frege (1969a, p. 16-17) remarked. Second, Boole's conception prevented him from bringing about "an organic connection" between the two parts. He didn't employ the equations of his first part as constituents of equations of the second part, as Frege noticed (1969a, p. 17).

Frege's supposed appeal to epistemological criteria.

With his appeal to the methodological criteria of simplicity, economy, significance, scope, and coherence, Frege did not endorse the thesis that logical analysis cannot be done without ontological commitment. If this means that we can find Van Fraassen's first thesis implicit in Frege, then, provided that we conceive the "orthodox logical theory" as Boolean logic, ascribing the second and third theses to Frege will also yield no problems. However, Frege added one consideration which seems to undermine this position. It has to do with Frege's break with the logical tradition that judgments have to be composed out of a subject and a predicate. In a remark in Begriffsschrift, Frege said that in his first draft of a formula language, he was led astray by the model of language in forming judgments out of subject and predicate - but that he soon came to be convinced that this was an obstacle to his special goal and led only to useless prolixities. So he replaced the subject-predicate distinction by an argument-function distinction.

The introduction of this logical distinction in Begriffsschrift did not present difficulties; Frege chose a simple example as a starting point: the fact that hydrogen is lighter than carbon dioxide, expressed in his symbolic language (Formelsprache). The idea is that when the symbol for hydrogen is replaced here by a symbol for, say oxygen, the original expression of a conceptual content is divided into a constant component, which "represents the totality of relations", and the symbol which is regarded as replaceable and denotes "the object which stands in these relations". The first component is called a function, the second its argument. Clearly, expressions are what are called function or argument, but the relations and objects for which functions and relations stand are not left out of consideration. (Frege would later distinguish between an expression of a function and the function itself, and the symbol for an argument and the argument itself; I shall discuss this below.) Moreover, a function (the expression!) can also be regarded as a replaceable symbol, and therefore an expression $\Phi(A)$ can be conceived not only as a function of the argument A, but also as a function of the argument Φ. In general, in the expression of a judgment within the symbolic language conceived by Frege, the combination of symbols to the right of the assertion sign can always be regarded as a function of one (or more) of the symbols occurring in it. The fruitfulness of this idea appeared as soon as a theory of substitutional quantification was given. (Cf. Frege 1879a, p. 19; p. 60.)

The distinction between function and argument, as outlined in the above paragraph, has the advantage that it is effective in concept formation (1879a, p. VII). For example, the concept "fourth root of 16" arises from the assertible content of '$2^4 = 16$' by imagining the symbol '2' as replaceable. In a similar way, one obtains the concept "logarithm of 16 on the base 2". This can be seen as a procedure in which concept formation takes place "after" a judgment has been made, literally (1969a, p. 17):

> Das Bilden der Begriffe lasse ich erst aus den Urtheilen hervorgehen.

This is alright so long as one sees it as a logical procedure, which has nothing to do with the controversy whether the concept or the judgment has epistemological or even ontological priority, an issue on which several nineteenth century logicians took a stand (cf. Ulrici's Compendium der Logik). But Frege himself might be accused of shifting from logical priority to a sort of epistemological priority when he encountered a kind of paradox in his alternative of breaking up the assertible content in order to gain the concept (1969a, p. 18-19):

> Allerdings muss der Ausdruck des beurtheilbaren Inhaltes, um so zerfallen zu können, schon in sich gegliedert sein. Man kann daraus schliessen, dass mindestens die nicht weiter zerlegbaren Eigenschaften und Beziehungen eigne einfache Beziehungen haben müssen. Daraus folgt aber nicht, dass losgelöst von den Dingen die Vorstellungen dieser Eigenschaften und Beziehungen gebildet werden; sondern sie entstehen zugleich mit dem ersten Urtheile, durch das sie

Dingen zugeschrieben werden.

This serves as an explanation why in the Begriffsschrift symbols for properties and relations do not appear separately, but always in connections which express assertible contents. More precisely (o.c.):

> Ein Zeichen einer Eigenschaft erscheint nie, ohne dass ein Ding wenigstens angedeutet wäre, dem diese Eigenschaft zukäme, die Bezeichnung einer Beziehung nie, ohne Andeutung der Dinge, die in ihr ständen.

Here we have an argument that is totally different from the above-mentioned methodological discussions. What is so strange, is that Frege here declares that his conceptual notation is related to the way in which ideas arise. I have only one explanation for this - I venture to say - un-Fregean excursion. There had been a sympathetic review of Begriffsschrift by Lasswitz in the Jenaer Literaturzeitung. Lasswitz had accused the Boolean logicians of a one-sidedness because they did not sufficiently consider "the real nature and formation of concepts in their relation to deducing and judging". (I quote from the translation given by Bynum 1972a, p. 210.) On the other hand, he praised Frege for "a series of very penetrating and significant remarks about logical and epistemological concepts". According to Lasswitz, "the apprehension of the judgment as a unified whole which is independent of the linguistic distinction of subject and predicate, is a conclusion which confirms anew the epistemological results already obtained in some other way" (o.c., p. 211-212). It seems as if Frege sought support from that side when he wrote the foregoing elucidation - an explanation which tallies with the fact that Frege also pointed out in a note that some philologists considered the "sentence word" (a word in which a whole judgment is pronounced) to be the primitive form of speech (Frege 1969a, p. 19). I would prefer to say that Frege used the findings of epistemologists and philologists as a (dubious) support for an aspect of his system, rather than that his procedure - in which concept formation takes place after a judgment - implies "an epistemic claim concerning the contents of judgments" in the sense that they are epistemically primary.

This epistemic priority view is argued for by Hans Sluga in his book Gottlob Frege (1980a, p. 92). He based it on the following quotation from a letter of Frege, presumably to Stumpf (1976a, p. 164):

> Ich glaube nun nicht, dass das Bilden der Begriffe dem Urtheilen vorausgehen könne, weil das ein selbständiges Bestehen des Begriffes voraussetzte, sondern ich denke den Begriff entstanden durch Zerfallen eines beurtheilbaren Inhaltes. Ich glaube nicht, dass es für jeden beurtheilbaren Inhalt nur eine Weise gebe, wie er zerfallen könne, oder dass eine der möglichen Weisen immer einen sachlichen Vorrang beanspruchen dürfe.

I do not see how this quotation could be taken as containing an epistemic claim, since the nature of knowledge is nowhere at issue. But doesn't it support an ontological evaluation in the sense that Frege's procedure implies that assertible contents are ontologically primary? In my view, Frege did not appeal here to a certain "ontology". He merely meant that a concept cannot be seen apart from the fact that something falls under it. Indeed, as can be seen in the above-mentioned letter, Frege considered as essential for the concept that "the question whether something falls under it has a meaning", a formulation which he also used in "Booles rechnende Logik und die Begriffsschrift", as we have seen (1969a, p. 20). But in his letter he went one step further (1976a, p. 164):

> Der Begriff ist ungesättigt, indem er erwas fordert, was unter ihn falle; daher kann er nicht für sich allein bestehen. Dass nun ein Einzelnes unter ihn falle, ist ein beurtheilbarer Inhalt, und der Begriff scheint dabei als Prädikat und ist immer prädikativ.

What is more, he didn't recognize the copula as a relation between an individual subject and a predicate:

> In diesem Falle, wo das Subjekt ein Einzelnes ist, ist die Beziehung von Subjekt und Prädikat nicht ein Drittes, das zu beiden hinzukommt, sondern sie gehört zum Inhalte des Prädikates, wodurch dieses eben ungesättigt ist.

Now there can be little doubt that Frege saw the subject-predicate distinction as a linguistic or grammatical distinction which as such was not useful as a logical distinction. With Sigwart (1873a, p. 91-92), he was aware of linguistic phenomena like the occurrence of a copula or verb-endings (1969a, p. 101), for he called them the linguistic expression for the peculiarity of a thought. He was willing to call a concept which is not a relation a "predicate" - and the object which falls under that concept a "subject" - provided one doesn't ask for a logical counterpart of the linguistic phenomenon of the copula. On the other hand, Frege saw his object-concept distinction as a logical distinction, "without which it is impossible to express the particular and existential judgments appropriately and in such a way that their close kinship catches the eye" (1976a, p. 165). The only question is whether Frege saw the concept-object distinction also as an ontological distinction, in the sense that it was "intended to reflect what Frege took to be the logical structure of reality" ...

The last quotation is from Kluge (1980a) who wrote a whole book on "the metaphysics of Gottlob Frege", with the sub-title "an essay in ontological reconstruction". Kluge's aim was to demonstrate that metaphysical theses constituted an integral part of Frege's overall philosophical effort (cf. Kluge 1980a, p. 5). This amounts to saying that, for Frege, logical analysis <u>cannot</u> be done without ontological commitment through the use of language. It is thus incompatible to (T1) on p. ix-x.

Kluge's argument on Frege's supposed metaphysics

Discussions about Frege's alleged ontology or metaphysics date from the time that Wells wrote his well-known paper "Frege's ontology". Its thesis was that Frege's semantical doctrines have ontological implications in the sense that they contribute to the description of the major kinds of being (cf. Wells 1951a, p. 540). Notably Bergmann and Klemke agreed with Wells that Frege was an ontological philosopher, though they drew different conclusions about Frege's "true" ontological position.

I do not intend to discuss these older views on Frege - on the whole both Bergmann and Klemke reached their conclusions on the basis of premises for which almost no evidence was given from the writings of Frege himself. Bergmann said that two things about Frege are "beyond reasonable doubt": first, that "he would have agreed that everything he calls an object is an existent", and second, that "his distinctions are so sweeping indeed that, if the word is to have any meaning at all, one cannot but call them ontological" (Bergmann 1958a, p. 438-439). Klemke remarked that he could not find any evidence in Frege's writings for Bergmann's identification of "object" with "existent". Instead, he granted ontological status to everything which Frege called the reference (Bedeutung) of a name. On this basis, Klemke could argue that Fregean "concepts" had ontological status. However, the quotations purported to support this view centre round Frege's remark in his "Kritische Beleuchtung einiger Punkte in E. Schröders Vorlesungen über die Algebra der Logik" that "the concept is logically prior to its extension" (Klemke 1959a, p. 511-512).

I shall not discuss Dummett's more recent support for the thesis that Frege was an ontologist, since he based his view on a wrong translation of a remark in Function und Begriff, to wit, "the distinction between functions of first and second level is not thereby banished from the world, because it is not made arbitrarily, but founded deep in the nature of things" (Dummett 1981b, p. 429). Frege wrote: "Damit ist aber der Unterschied zwischen Functionen erster und zweiter Stufe nicht aus der Welt geschafft, weil er nicht willkürlich gemacht, sondern in der Natur der Sache tief begründet ist." (Frege 1891a, p. 31). But I do want to discuss Kluge's views because he cannot be accused of not having read Frege thoroughly.

One of the pillars on which Kluge's argumentation rests is a passage from Frege's later essay "Ueber die Grundlagen der Geometrie" (1903b) concerning the object-concept distinction (Frege 1903b, p. 371; my italics):

> Nehmen wir den Satz "Zwei ist eine Primzahl". Wir unterscheiden hier sprachlich ein Subjekt "Zwei" und einen prädikativen Bestandtheil "ist eine Primzahl".(...) Der erste Bestandtheil "Zwei" ist ein Eigenname einer gewissen Zahl, bezeichnet einen Gegenstand, ein Ganzes, das keiner Ergänzung mehr bedarf. Der prädikative Bestandtheil dagegen "ist eine Primzahl", bedarf der Ergänzung, bezeichnet keinen Gegenstand. Ich nenne den ersten Bestandtheil auch gesättigt. *Diesem Unterschiede in den Zeichen entspricht natürlich ein solcher im Reiche der Bedeutungen: dem Eigennamen der Gegenstand, dem prädikativen Theile etwas, was ich Begriff nenne*.

The conclusions lie on the surface: object and concept are two ontological categories - the difference between objects and concepts is that the first are independent, the second dependent entities; there is not a third category of so-called nexus; when one dives further into Frege's work, one discovers that concept is a sub-category of the category of function, which also comprises relations and mathematical functions; among the objects, we not only encounter spatio-temporal objects, but also times, places, mathematical objects, ideas and - since *Function und Begriff* - also so-called truth values, value ranges and senses - *Sinne* - (cf. Wells, "Frege's ontology", 1951a). So how can I conclude that Frege's way of doing logic was consistent with Van

Fraassen's first thesis that philosophical logic can be done in such a way as to involve no ontological commitment through any use of language?

In this section, I restrict the discussion to showing that Frege's way of cultivating logic was not different from his way of doing mathematics. For Frege, logic was a science which aims for finding the logical laws that govern correct deductions (1969a, p. 5). According to Frege, there is an immense multitude of such laws and this impels us to look for a number of axioms from which all the other laws can be deduced (1879a, p. 25). The same holds mutatis mutandis for the deduction rules, which cannot be expressed in the conceptual notation itself, because they form its basis, like the syntactical rules. Distinctions and a terminology for the formulation of these axioms and rules are needed. Some, but not all of these terms can be defined; if not, then one has found a primitive distinction or terminology - at least for the time being (1892b, p. 193). Now the problem arises whether each user means the same thing with the same primitive symbol - mit demselben Zeichen (Worte) dasselbe bezeichnet. In order to achieve this, the scientist must give an informal explication - Erläuterung - , often in pictorial language; it goes without saying that such explanations have no place in the system of science, unlike the definitions; the significance of the latter lies in the logical construction out of the primitive signs (1906a, p. 303); once scientists have come to agree about the primitive signs and what they mean, an agreement on the logical complex - das logisch Zusammengesetzte - is easily reached (1906a, p. 301).

Frege spoke sometimes about primitive symbols, but also about their indications - Bezeichnungen - or their meanings - Bedeutungen - . He used the word 'Bedeutung' in connection with primitive symbols as well as for defined signs. About definitions, Frege (1906a, p. 302) said:

> Sie setzen natürlich die Kenntnis gewisser Urelemente und ihrer Zeichen voraus. Aus solchen Zeichen setzt die

> Definition eine Gruppe von Zeichen rechtmässig zusammen, so dass die Bedeutung dieser Gruppe durch die Bedeutungen der benutzten Zeichen bestimmt ist.

Contrary to Kluge (1980a), I do not believe that Frege's way of speaking in itself involves an ontological commitment. For how are the terms 'Bezeichnungen' and 'Bedeutungen' used? We have seen that Frege already used the term 'Bedeutung' in his comparison of his conceptual notation with Boole's system. Parts of the discussion centred around the signs for the logical connections - Zeichen für logische Beziehungen - in the different systems. There is nothing unusual or philosophical about speaking of signs for relations: the same use of language occurs in mathematics, as can be seen from the following quotation, taken at random from a nineteenth century textbook, Die Differential- und Integralrechnung mit Functionen einer Variablen (1839a) by Raabe:

> Will man im Allgemeinen irgend eine Function einer allgemeinen Grösse x andeuten oder bezeichnen, so setzt man derselben einen der Buchstaben f, F, ϕ, ψ, ... vor. Es stellen demnach die Symbole $f(x)$, $F(x)$, $\phi(x)$, $\psi(x)$, ... beliebige, algebraische oder transcendente Functionen der allgemeinen Grösse x dar.

In theory, Raabe distinguished between function symbols and functions; the latter are algebraic performances: "Jede algebraische Verrichtung (functio) mit einer allgemeinen Grösse wird eine Funktion dieser Grösse genannt." As a right-minded mathematician he didn't bother about the ontological or epistemological status - if any - of those functions. Neither did Frege in regard to logical connections in his Boole-articles. Moreover, his way of speaking in these papers is not very different from his use of words in the series of articles on the foundations of geometry, in spite of the fact that Frege used the term 'Bedeutung' also in a more or less technical sense since 1891. This can be seen from Frege's elucidation of the last statement of the following passage (1903b, p. 319-320; my italics):

> Definitionen nennt man in der Mathematik wohl allgemein die Festsetzung der Bedeutung eines Wortes oder Zeichens. Die Definition unterscheidet sich von allen andern mathematischen Sätzen dadurch, dass sie ein Wort oder Zeichen enthält, das bis dahin keine Bedeutung hatte, nun aber durch sie eine bekommt. *Alle andern mathematischen Sätze (axiomatische und Lehrsätze) dürfen keinen Eigennamen, kein Begriffswort, kein Beziehungswort oder Funktionszeichen enthalten, dessen Bedeutung nicht schon vorher feststände*.

Frege's critic, Korselt, had discussed the question why the axioms and derived propositions - *Lehrsätze* - must not contain signs, of which the "meaning" is not determined beforehand, but he read the expression 'Das Zeichen hat keine Bedeutung' (wrongly) as 'Uns sind keine Sätze bekannt, die den Gebrauch dieses Zeichens überhaupt oder in gegebenen Gebiete regeln' (Korselt 1903a, p. 402).
Frege (1906a, p. 298) explained the matter - in such a way that later (metaphysically oriented) writers would immediately draw their conclusion of "ontological commitment" - :

> Ich hatte mir die Sache viel einfacher gedacht, nämlich so: ein Eigenname hat im wissenschaftlichen Gebrauche den Zweck, einen Gegenstand zu bezeichnen, und dieser Gegenstand ist, falls der Zweck erreicht wird, die Bedeutung des Eigennamens. Entsprechend ist es bei den Begriffszeichen, den Beziehungszeichen, den Funktionszeichen. Die bezeichnen beziehungsweise Begriffe, Beziehungen, Funktionen, und das, was sie bezeichnen, ist dann ihre Bedeutung.

But here we have the correct way of speaking of ordinary mathematicians who are interested in mathematical concepts, relations and functions and examine their properties without raising ontological or epistemological questions. In a similar way, logicians study logical concepts, relations and functions. Among the logical concepts are those of concept, relation and function themselves (cf. Frege 1884a, p. 83). We look in vain for ontological or epistemological discussions of logical concepts; at most, there are informal explications - Erläuterungen - of the primitive logical concepts and definitions of the rest. For, as we know, a definition for the introduction of a name for the logically simple is not possible. The only possibility is to give the reader or hearer hints in order to understand what is meant by the word (Frege 1892b, p. 193). In short, there is no evidence in Frege's writings that concept, relation, etc. are for him ontological categories. He took it for "a sure indication of a mistake when logic needs metaphysics and psychology, sciences which require the logical principles themselves" (1893a, p. XIX). So why did Frege stress the point of what his symbols signify (indicate) at all?

One answer is that in his time, a sort of philosophy of mathematics seemed to have been developed in which not only functions but also numbers were identified with (their) expressions. It is true that early writers like Euler characterized a function of x as an (analytical) expression in x - that is, any expression which is composed out of powers, logarithms, trigonometric functions and so on (cf. Klein 1933a, p. 216). But they were no formalists; mathematics was considered as a science of quantities, not of signs. This presented problems in the case of the so-called imaginary quantities; the man who can be considered the creator of the theory of functions of a complex variable, Cauchy, went so far as to conceive the sign '$\sqrt{-1}$' as a mere instrument for calculations, signifying nothing and having no sense (Cauchy 1844a, p. 361).

This produced a reaction; according to Durège (1864a, p. 3) the view of the impossibility of the imaginary quantities rested on a misunderstanding of the nature of negative, fractional and irrational quantities: the true nature of these mathematical concepts was seen in any of their applications - in geometry, mechanics, physics and even partly in civil life; "now for imaginary quantities such an application was not near at hand and because of the deficient knowledge thereof one saw oneself forced to relegate the imaginary quantities to the realm of impossibility - das Reich der Unmöglichkeit - and to doubt their existence". So Durège formulated a (new) philosophy of mathematics which was meant to solve the problem of existence in pure mathematics: "its concepts, introduced by a complete and contradiction-free definition found their existence in the definition itself". The definitions are not arbitrary: they are the necessary consequence of the so-called principle of permanence, which was probably first formulated by Durège (1864a, p. 9). He was followed in this by Hankel (1867a) and - according to Kossak (1872a) - also by Weierstrass.

The problem is, of course, how freedom of contradiction can be established. Frege, who discussed this problem extensively on several occasions, said that he saw no suitable principle other than that properties found in the same object (!) should not be inconsistent with each other. But then, if one had such an object, the formal theory would be superfluous (1885a, p. 103). Heine cut the Gordian knot by calling the number signs themselves "numbers". (The relevant quotation can be found in Frege's second volume of Grundgesetze der Arithmetik (1903a, p. 97).) Without mentioning any adherent of this "formal theory", Frege ridiculed it in his talk "Ueber formale Theorien der Arithmetik" (1885a, p. 97ff) by pointing out that no geometrical, physical or chemical property of, say, the sign '$\frac{1}{2}$' makes adding $\frac{1}{2}$ to itself result in 1; but if, as the formalist demands, one stipulated this by a definition, then one could also stamp one's fellow-citizen as a liar by the simple means of a definition ...

So Frege distinguished between signs or symbols - Zeichen - and what they signify or symbolize - bezeichnen - , just as every mathematician did before the formalist theory had arisen. He emphasized this in the context of the foundations of mathematics, because of the existence of formalist theories. (Before such "theories" had come into existence, mathematicians did not need to put expressions between (double) quotes, when the expressions themselves were meant. In his "Logische Mängel in der Mathematik" (1969a, p. 72) Frege made it clear that Riemann's way of speaking, e.g. that $_a\int^b f(x)dx$ - in stead of ' $_a\int^b f(x)dx$ ' - has no meaning when the corresponding limit does not exist, was not problematic in his time, when "the mathematical disease of the day - Zeitkrankheit - of mixing the signs with the signified had not expanded that much". Clearly Frege did not consider Riemann a formalist.)

Consequently, when Frege (1903b, p. 372) explained to Hilbert his distinction between first and second level concepts via his distinction between concepts and objects, he could not appeal to the difference in the symbols. He had to refer to the corresponding difference in the realm of the meanings - im Reiche der Bedeutungen - . By calling the indications of some signs 'Gegenstände', Frege fixed their logical function, for example:

> Ein Gegenstand - z.B. die Zahl 2 - kann an einem andern Gegenstande - z.B. Julius Cäsar - logisch gar nicht haften ohne ein Bindemittel, das aber kein Gegenstand sein darf, sondern ungesättigt sein muss.

There is another reason why Frege had to talk about the indications instead of about the symbols. Natural languages are not unambiguous; the same symbol can have different logical functions in different contexts: the same word may serve to signify a concept, as well as a single object which falls under that concept (Frege 1882a, p. 50). How could one explain a difference in symbols without appealing to their significations? Not that speaking about objects and concepts is an easy business; often one must speak in figurative language in order to make clear the primitive (logical) distinctions. Such elucidations have an illuminating function; one need not take them as philosophical statements, for reasons mentioned before. Frege was very conscious of this: in the second part of his discussion with Hilbert (1903b, p. 372) he twice pointed out that he expressed himself in a figurative way, first when he employed the terms 'gesättigt' and 'ungesättigt', second when he talked about different "logical places" in some of which only objects but no concepts, in others only concepts and no object can "stand". (More examples will follow when Frege's second logical system is discussed.)

Now we can return to Kluge's conclusions. According to him, "object and concept are two ontological categories, whereas the difference between object and concept is that the first are independent, the second dependent entities, while there is no third category of so-called nexus". However, he omits the following facts: (1) Frege always spoke of logical distinctions, and (2) Frege admitted that qualifications as "dependent" and "independent" are metaphorical. Now there have been and still are people who are engaged in ontology or metaphysics as "a systematic investigation of the most general structure of reality". But nowhere in Frege's publications from 1879 till 1903 is there an indication of that sort of activity. As Sluga (1976a, p. 29) pointed out, "to approach Frege's thought from an ontological point of view is to approach it completely unhistorically". Frege's talk of "objects" and "concepts" was such that it had no ontological interpretation. When Frege called e.g. the number 2 an object, this implied that the question whether anything falls under it, is meaningless (1969a, p. 20; 1884a, p. 64). Here there seems to be no other possibility than to interpret this as a logical matter. Nevertheless, Frege (1884a, p. 72) was at least on one occasion careful to warn against possible misunderstandings, namely when there had been talk of the independence of numbers:

> Die Selbständigkeit, die ich für die Zahl in Anspruch nehme, soll nicht bedeuten, dass ein Zahlwort ausser dem Zusammenhange eines Satzes etwas bezeichne, sondern ich will damit nur dessen Gebrauch als Praedicat oder Attribut ausschliessen, wodurch seine Bedeutung etwas verändert wird. [9]

Until now, I have not discussed Frege's "special goal" (1879a, p. 4.) of his conceptual notation: the rigorous treatment of arithmetic.

Within this project, conditions which are required "from the side of logic and for the sake of the rigour of proof are imposed upon the introduction of symbols for objects and concepts" (1884a, p. 87). The presence of those conditions makes it even more dubious to look upon the object - concept distinction as an ontological distinction. Though Frege introduced these conditions already in Die Grundlagen der Arithmetik, it is only in Grundgesetze der Arithmetik that their role becomes clear. Thus, I will treat them in the next chapter which is devoted to Frege's second system.

In this chapter I argued: (1) Frege gives only methodological arguments in his defense of his early logical theory; (2) the means which he uses in order to make this theory comprehensible have no theoretical significance and have no ontological significance either, but serve only a didactical purpose; and (3) Frege's talk about functions etc. as distinct from function expressions etc., has no more "ontological content" than the usual way in which mathematicians speak.

CHAPTER TWO

FREGE'S SECOND SYSTEM

Frege's reconstruction of arithmetic as a topic in philosophical logic

In the special situation of a formal axiomatization of arithmetic, Frege's approach required innovations and complications on the side of the formal apparatus. Additions and new versions - Ergänzungen und neue Fassungen - were necessary with respect to the original whole of notations called "Begriffsschrift" (1891a, p. 2). It is true that there seems to be no reason to concentrate on the modified apparatus, as long as one considers Frege's rigorous treatment of artihmetic a topic outside the field of philosophical logic proper. But I believe that it accords with Frege's intentions when I interpret his treatment of arithmetic as a topic in philosophical logic. His original motivation for the subject was his wish to test how far one could come in arithmetic by means of deductions alone, supported only by the laws of thought which are beyond all particularities (1879a, p. IV). Frege was anxious to know this because he was not satisfied with Kant's answer to the philosophical question as to which sort of truths the arithmetical judgments belong. He therefore tried to "reconstruct" arithmetic on a purely logical basis. The similarity with the later discipline of logical reconstruction of world views is conspicuous: here too, the aim is to establish certain philosophical theses. Nevertheless we should not be misled by this similarity.

First, Frege's rigorous treatment of arithmetic is based on a result within the discipline of philosophical logic, namely a logical analysis of numerical assignments - Zahlangaben - . In Die Grundlagen der Arithmetik, Frege had come to the conclusion that every numerical assignment contained a statement about a concept (1884a, p. 59) and his treatment of arithmetic in his Grundgesetze der Artihmetik was dependent on this idea. The fact is however, that Frege made it appear here as if he had reached this conclusion in isolation from ordinary language and only afterwards had seen that it was _also_ valid there (1893a, p. IX). But an examination of the pertinent discussion in _Die Grundlagen der Arithmetik_ shows that Frege developed his view from certain basic numerical judgments in German (cf. 1884a, par. 46). In current jargon, Frege tried to discover the logic underlying language games about numbers, and used his acquired insight to establish the logical or formal nature of arithmetics. He had already reduced the arithmetical concept of ordering in a sequence to the notion of _logical_ order (1879a, p. IV), but he had not yet made the promised further step to the number concept. This was made possible by his later results; and here we encounter another argument why Frege's reconstruction of arithmetic cannot be seen as a contribution _avant la lettre_ to the programme of logical reconstruction of world views, but has to be seen as an elaboration of logic itself! For, in Frege's view it was a purely logical business, quite different from, say, a reconstruction of geometry. As a matter of fact, the contrast between arithmetic and geometry was the starting point when Frege expounded his view of arithmetic (1885a, p. 94-95): Geometry needs certain geometrical axioms, the negation of which would be possible, that is, without contradiction, from a purely logical standpoint. On the other hand arithmetic does not have special arithmetical axioms; this claim is plausible on account of the universal applicability of the arithmetical doctrines:

> In der That kann man so ziemlich alles zählen, was Gegenstand des Denkens werden kann: Ideales so gut wie Reales, Begriffe wie Dinge, Zeitliches so gut wie Räumliches, Ereignisse wie Körper, Methoden so gut wie Lehrsätze; auch die Zahlen selbst kann man wieder zählen. Es wird eigentlich nichts verlangt als eine gewisse Schärfe der Abgrenzung, eine gewisse logische Vollkommenheit.

The first conclusion to be drawn is this: the fundamental principles of arithmetic must extend to everything thinkable and these most general propositions are rightly assimilated to logic : "one can even impress upon the logicians that they cannot come to know their own science thoroughly, when they ignore arithmetic" (1885a, p. 95). The second conclusion is that there are no specifically arithmetical deduction rules which cannot be reduced to the general ones of logic. For if such a reduction were impossible with regard to a certain rule, then the question would rise about the ground of justification - Erkenntnissgrund - of its correctness. But the universal applicability of arithmetic excludes spatial intuition as well as physical observation, so nothing remains other than to recognize the purely logical nature of arithmetical forms of deduction too. Frege claimed to have shown this for a case in which this could not be seen immediately, namely mathematical induction (1885a, p. 96).

To sum up: Frege tried to give an axiomatical characterization of the structure of those logical deductions which are called calculations. (The latter formulation was suggested by Frege (1884a) himself (p. 99) [10].)

Mathematical generalization: a major technique

It seems wise to treat first of all - what Wells (1951a) called - one of Frege's major techniques, mathematical generalization. This principle as such gives no rise to ontological implications, as its mathematical origin indicates. Frege applied it to the meaning of the word 'function'. This had already been extended during "the progress of science", for in the development of higher mathematical analysis, not only the way in which functions are formed had been extended, but also what can appear as argument and as value (cf. Frege 1891a, p. 12-13). Frege proceeded further into this direction: his concept of a function in his Begriffsschrift was already less restricted than that in mathematical analysis, because every expression - whether or not it concerned an assertible content - could be taken for forming functions by introduction of variables (Frege 1879a, par. 9-10). In Function und Begriff, the same procedure is chosen without blurring the distinction between function expressions and functions. It is true that functions were called "incomplete, in need of completion or unsaturated" - unvollständig, ergänzungsbedürftig oder ungesättigt - but, as we have seen, this is only a metaphorical way of speaking. Actually the expression of a function is unsatisfied or in need of completion (Frege 1893a, p. 5). The concept of function is logically primitive, so Frege could not give any definition but had to elucidate what he meant with the word 'function'. The same holds for what can be considered arguments and values of functions; Frege's urge for generality went as far as choosing another logically primitive concept, that of "object" - Gegenstand - . The elucidation is short and snappy (1891a, p. 18):

> Gegenstand ist alles, was nicht Function ist, dessen Ausdruck also keine leere Stelle mit sich führt.

Frege called such "saturated" expressions proper names - Eigennamen - . In accordance with how one is engaged in logical analysis of arithmetic or of a natural language, different sorts of expressions count as proper names; in arithmetic e.g. '2+2', in the German language e.g. 'Caesar' and 'die Hauptstadt des Deutschen Reiches' and, as we shall see below, also full sentences, for example '2=4' and 'Caesar eroberte Gallien'. Leaving aside for the moment the question of the sentences, we see that not only numbers, but also persons are "objects". This seems a bit strange for a native speaker of German (c.q. English) and Frege did not hesitate to draw attention to it when he discussed what can appear as an argument of a function (1891a, p. 17):

> Es sind nicht mehr bloss Zahlen zuzulassen, sondern Gegenstände überhaupt, wobei ich allerdings auch Personen zu den Gegenständen rechnen muss.

But there is no indication that Frege had the illusion of creating a (Meinongian?) Gegenstandstheorie when he wrote this. Clearly persons, and also places, times and spaces are objects from a logical point of view (1892a, p. 42; my italics):

> Oerter, Zeitpunkte, Zeiträume sind, logisch betrachtet, Gegenstände; mithin ist die sprachliche Bezeichnung eines bestimmten Ortes, eines bestimmten Augenblicks oder Zeitraums als Eigenname aufzufassen.

Would Frege have written these lines if he had believed that objects form an ontological category? That he did not draw ontological consequences becomes still more evident when he commented on the fact that empty places do not occur in an assertive sentence - Behauptungssatz - so that such a sentence also has to be considered a proper name. A proper name of what? Frege's answer was that all true sentences are proper names of a "truth value", a terminology in which the relation with values is retained. (In Function und Begriff Frege introduced truth values with the help of the function $x^2 = 1$, where x represents the argument (1891a, p. 13):

> Ich sage nun: "der Werth unserer Function ist ein Wahrheitswerth" und unterscheide den Wahrheitswerth des Wahren von dem des Falschen. Den einen nenne ich kurz das Wahre, den andern das Falsche.

In "Ueber Sinn und Bedeutung" Frege simply said that "these two objects are acknowledged at least implicitly by everyone who makes any judgment at all, holds anything as true, thus even by the sceptic".

It can be admitted that everyone at least implicitly believes that some sentences are false and others true, but Frege's speaking of truth values as objects must have sounded unfamiliar to those readers who had not read Function und Begriff. It seems as if Frege realized this when he added that to designate truth values as objects might appear as a mere play on words from which no decisive conclusions should be drawn (1892a, p. 34). He referred to another essay, presumably "Ueber Begriff und Gegenstand", where no ontological or epistemological, but only logical and linguistic arguments are presented. However, in "Ueber Sinn und Bedeutung", Frege went further by bringing in the question of objectivity (1892a, p. 34):

> Aber so viel möchte doch schon hier klar sein, dass in jedem Urtheile* - und sei es noch so selbstverständlich - schon der Schritt von der Stufe der Gedanken zur Stufe der Bedeutungen (des Objectiven) geschehen ist.
>
> * Ein Urtheil ist mir nicht dass blosse Fassen eines Gedankens, sondern die Anerkennung seiner Wahrheit.

The fact that Frege, in the same paragraph, spoke about truth values as objects and about the level of the objective seems to fuel the opinion that Frege "sought to protect objectivity in science by grounding his basic categories ontologically, by adopting, that is, an extreme form of platonic realism". This position was taken by Bell (1979a, p. 73-74) who wished to explain why it is "that not only concrete objects, but (also) abstract objects, functions, senses and truth-values are accorded ontological status as a means of protecting their objectivity against the pernicious encroachment of philosophical idealism and scientific psychologism" (o.c., 74). Bell even went so far as to claim that Frege's programme "produces ontologically unacceptable (if intelligible) results, such as a truth value's being an object composed of a complete and an incomplete part" (o.c., p. 74).
Bell's position has to be taken seriously, because it gives rise to a number of important questions; they will be discussed in the following section.

Bell's claim of ontological implications of Frege's second system rejected

David Bell (1979a) raised objections especially against Frege's second system. I pose three questions concerning aspects of Frege's second system that seem relevant to Bell's negative conclusions. The answers to these questions will enable us to take a stand on Bell's position.

(1) Which further principles guided Frege when he changed his first system? This question is particulary important where Bell argues that "Frege's programme breaks down when applied to the language of ordinary discourse" (Bell 1979a, p. 74). (As we shall see, Bell did not accept that statements of an ordinary language, such as 'Caesar is a prime number' which he finds nonsensical, are to be assigned a truth value.)

(2) Is it possible to make Frege's remarks about truth values comprehensible, including the - admittedly obscure - comment that judging comes down to the discerning of parts within the truth value (Frege 1892a, p. 35)? These and similar remarks indeed seem especially suited to ascribing an ontological position to Frege.

(3) What is the nature of Frege's objectivity thesis, given his confession that he recognized a realm of the objective, non-real (Frege 1893a, p. XVIII)? Is Bell not in good company when he attributed to Frege a so-called Platonic ontology on the basis of this thesis? Russell already seems to have identified Frege's realm of the objective, non-real with his own "world of being" (cf. Russell 1912b, p. 156) in his article "Is position in time and space absolute or relative?" (Russell 1901b, p. 312). And did he not write in his "introduction to the second edition" of The principles of mathematics: "At the time when I wrote the "Principles", I shared with Frege a belief in the Platonic reality of numbers, which, in my imagination, peopled the timeless realm of Being" (Russell 1973a, p. ix-x)? [11]

The first of the above questions will be dealt with in this section. Answers to the second and the third question will be given in the two following sections. To answer the first question, an examination of the foundations of Frege's second system is required. In Die Grundlagen der Arithmetik (1884a), two conditions appear which shape Frege's second system in many details. The first settles the admissability of saturated expressions or names of objects (p. 73):

> Wenn uns das Zeichen a ein Gegenstand bezeichnen soll, so müssen wir ein Kennzeichen haben, welches überall entscheidet, ob b dasselbe sei wie a, wenn es auch nicht immer in unserer Macht steht, dies Kennzeichen anzuwenden.

The second tells us when we have a logically admissible concept (Frege 1884a, p. 87):

> Alles was von Seiten der Logik und für die Strenge der Beweisführung von einem Begriffe verlangt werden kann, ist seine scharfe Begrenzung, dass für jeden Gegenstand bestimmt sei, ob er unter ihn falle oder nicht.

The first condition appears earlier in connection with number expressions. Asking for definitions of '0' and '1', Frege attempted to introduce the expressions 'die Zahl 0' and 'die Zahl 1' by means of abbreviations, for example (p. 67):

> einem Begriffe kommt die Zahl 0 zu, wenn allgemein, was auch a sei, der Satz gilt, dass a nicht unter diesen Begriff falle.

But, as he remarked, it only _seems_ as if we have explained the 0 and the 1; in reality we have only determined the meaning of the expressions 'die Zahl 0 kommt zu' und 'die Zahl 1 kommt zu'; but it does not allow discrimination of the 0 and the 1 as independent, recognizable objects - _als selbständige wiedererkennbare Gegenstände zu unterscheiden_ - (p. 68). About these and similar (recursive) definitions with the help of 'dem Begriffe F kommt die Zahl (n+1) zu', he also says:

> wir können - um ein krasses Beispiel zu geben - durch unsere Definitionen nie entscheiden, ob einem Begriffe die Zahl _Julius Caesar_ zukomme, ob dieser bekannte Eroberer Galliens eine Zahl ist oder nicht.

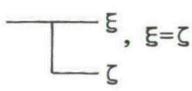
Thus, the first condition asks for a so-called recognition-judgment - *Wiedererkennungsurtheil* - (1884a, p. 119).

Eventually he succeeded in giving an explicit definition of the 0 as the extension of the concept "equinumerous - *gleichzahlig* - with the concept not-identical with itself" (p. 79-80, p. 87). But then he forgot to consider whether he had a criterion for deciding whether, say, Julius Cäesar is the same as 0 or not.

Such problems were treated in *Grundgesetze der Arithmetik*, where Frege started with a new system in which a generalized notion of extensions (*Umfänge*) - already introduced in *Function und Begriff* - was incorporated. The way in which this was done can be seen as an example par excellence of how Frege tried to satisfy his requirement of coherence. For he did not simply "add" a logic of extensions - *Umfangslogik* - to his earlier logical system, but reckoned with his so-called value ranges from the very beginning. In the following ten pages, I explain in detail how Frege tried to satisfy his criteria of precision, especially as regards to the introduction of value ranges. I begin with some remarks on notation. When Frege speaks about so-called first-level functions, he uses Greek letters ξ and ζ in order to mark the argument places for the so-called objects. When he speaks about second-level functions, he uses the Greek letter φ in order to mark the argument place for a first-level function with one argument.

The two primitive second-level functions are $\acute{\varepsilon}\varphi(\varepsilon)$ for the value range of first-level functions of the form $\varphi(\xi)$, and $\overset{a}{\smile}\!\!-\varphi(a)$ for the universal generalization of such functions. The primitive first-level functions with one argument are $-\xi$, $\top\xi$, $\backslash\xi$ (1893a, p. 48). These functions will be explained presently.

Compound names of first level functions are generated by taking a name of a primitive first-level function with two arguments and filling up one of the argument places with an object name. The only primitive first-level functions with two arguments are the following:



It was not enough for Frege to give the names of primitive functions alone, they had to be meaningful - <u>bedeutungsvoll</u> - in the sense that every object name obtained by taking suitable arguments, is also meaningful. This can be the case when such an object name signifies one of the objects which Frege had previously recognized, the True and the False. For the first function, $—\xi$, this is achieved by the following stipulation for the horizontal stroke (1893a, p. 9):

> Ich fasse ihn als Functionsnamen auf in der Weise, dass $—\Delta$ das Wahre ist, wenn Δ das Wahre ist, dass es dagegen das Falsche ist, wenn Δ nicht das Wahre ist.

Taking into account his characterization of a concept as a function with one argument that always has as value a truth value (1891a, p. 15; 1893a, p. 8), we can conclude that $—\xi$ is a logically admissible concept according to Frege's second condition of <u>Die Grundlagen der Arithmetik</u>, for (1893a, p. 9, n. 3):

> Die Festsetzung ist so getroffen, dass '$—\Delta$' unter allen Umständen etwas bedeutet, sofern nur 'Δ' etwas bedeutet. Sonst würde $—\xi$ kein scharfbegrenzter Begriff, also in unserm Sinne überhaupt kein Begriff sein.

Evidently, $—\xi$ is a concept under which only one object falls, namely the True. In the same way, Frege took $—\xi$ as a concept under which every object except one, the True, falls [12]. The third first-level function with one argument, $\backslash\xi$, cannot be introduced before more objects than the True and the False are introduced, but this is possible for the first-level function with two arguments,

Its value has to be False if the True is taken as its ζ-argument and <u>any object other than the True</u> is taken as its ξ-argument; in all other cases the function value has to be the True [13]. Clearly this function is a logically admissible connection - <u>Beziehung</u> - that is a function of two arguments that always has a truth value as value (1891a, p. 28; 1893a, p. 8).

The other primitive first-level function of two arguments,

$$\xi = \zeta$$

is also a logically admissible relation; of course, '$\Gamma = \Delta$' is a name of the True if Γ is the same as Δ, and in all other cases the function has to be the False. It could be objected that it is not clear what it means for Γ and Δ "to be the same" and this would bring us to Frege's first condition of <u>Die Grundlagen der Arithmetik</u>. As long as the truth values are the only accepted objects there is no problem. But when new meaningful object names are to be introduced, we have to show that they can also be taken as argument of these functions. To sum up (1893a,p. 48):

> Wir gehen davon aus, dass die Namen von Wahrheitswerthen etwas bedeuten, nämlich entweder das Wahre oder das Falsche. Wir erweitern dann allmählich den Kreis der als bedeutungsvoll anzuerkennenden Namen, indem wir nachweisen, dass die aufzunehmenden mit den schon aufgenommenen bedeutungsvolle Namen bilden, indem die einen an passende Argumentstellen der andern treten.

The notation '$\grave{\varepsilon}\varphi(\varepsilon)$' introduces an object name for every first-level function name with one argument. A name obtained in this way signifies something - <u>hat eine Bedeutung</u> - (1) when all names which arise from a meaningful first-level function name of one argument by taking this name as its argument signify something (1893a, p. 46) and (2) when all function names which arise from a meaningful name of a first-level function of two arguments, by taking this name as one of these arguments, signify something. Taking the most interesting example,

$$'\xi = \grave{\varepsilon}\varphi(\varepsilon)'$$

is a meaningful first-level function name when every proper name which arises from it by replacing 'ξ' by a name of a truth value or a name of the form '$\grave{\varepsilon}\varphi(\varepsilon)$' signifies something. This is achieved by three stipulations - Festsetzungen - :

'$\grave{\varepsilon}\Phi(\varepsilon)=\grave{\varepsilon}\Psi(\varepsilon)$' has to be synomynous with '$\smile^{\mathfrak{a}}\Phi(\mathfrak{a})=\Psi(\mathfrak{a})$', '$\grave{\varepsilon}(-\varepsilon)$' has to signify the True; and '$\grave{\varepsilon}(\varepsilon=(\top\smile^{\mathfrak{a}}\mathfrak{a}=\mathfrak{a}))$' the False.

In this way the admissibility of saturated expressions of the form '$\grave{\varepsilon}\varphi(\varepsilon)$' has been reached with the help of a condition, laid down in axiom V:

$$\vdash(\grave{\varepsilon}f(\varepsilon)=\grave{\alpha}g(\alpha))=(\smile^{\mathfrak{a}}f(\mathfrak{a})=g(\mathfrak{a}))$$

together with the identification of the True and the False in terms of value ranges. Frege gave almost no defense of these conditions; axiom V, or the possibility of transforming a general identity statement into an identity statement between value ranges, has to be regarded as a logical law. The only "excuse" which he gives is that this law always had been used when there was question of extensions of concepts. "The whole Leibniz-Boolean calculating logic is founded on it" (1893a, p. 14). Frege also appealed to its significance by recalling that in Die Grundlagen der Arithmetik he had defined number as the extension of a concept (1893a, p. 14).

Axiom V is an identity criterion for objects of the form '$\grave{\varepsilon}\varphi(\varepsilon)$' as requested by the first condition of Die Grundlagen der Arithmetik. Unfortunately, it is not a complete recognition-judgment (1893a, p. 16):

> Dadurch, dass wir die Zeichenverbindung '$\grave{\varepsilon}\Phi(\varepsilon)=\grave{\alpha}\Psi(\alpha)$' als gleichbedeutend mit '$\smile^{\!a}\!\!-\Phi(a)=\Psi(a)$' hingestellt haben, ist freilich die Bedeutung eines Namens wie '$\grave{\varepsilon}\Phi(\varepsilon)$' noch keineswegs vollständig festgestellt. Wir haben nur ein Mittel, einen Werthverlauf immer wiederzuerkennen, wenn er durch einen Namen wie '$\grave{\varepsilon}\Phi(\varepsilon)$' bezeichnet ist, durch welchen er schon als Werthverlauf erkennbar ist.

One way out is to characterize as value ranges the objects which are not "given" in the form of a value range, namely the truth values. And this is what Frege did in par. 10 of his <u>Grundgesetze</u>. This section is crucial for the question how Frege justified his introduction of value ranges. Scholz und Schweitzer (1935a, p. 102) considered it a paradigm of Frege's caution and precision - <u>ein Musterbeispiel Fregescher Vorsichtigkeit und Genauigkeit</u> - but as we know, Frege did not then see that this caution did not save him from inconsistency. He restricted himself to showing that it is always possible to let an arbitrary value range be the True and an arbitrary other value range the False without falling into contradiction with the identification of '$\grave{\varepsilon}\Phi(\varepsilon)=\grave{\varepsilon}\Psi(\varepsilon)$' with '$\smile^{\!a}\!\!-\Phi(a)=\Psi(a)$'. He did this as follows.

Let '$\grave{\eta}\Phi(\eta)=\grave{\alpha}\Psi(\alpha)$' be the same as '$\smile^{\!a}\!\!-\Phi(a)=\Psi(a)$' whereas none of the objects with a name of the form '$\grave{\eta}\Phi(\eta)$' are identical with one of the truth values. We assign to each object with a name of the form '$\grave{\eta}\Phi(\eta)$' and to each of the truth values a "new" object $X(\grave{\eta}\Phi(\eta))$ in the following way:

The value of $X(\xi)$ is

(1) an object with a name of the form '$\grave{\eta}\Phi(\eta)$', say $\grave{\eta}\Lambda(\eta)$ for the argument the True
(2) <u>another</u> object with a name of the form '$\grave{\eta}\Phi(\eta)$', say $\grave{\eta}M(\eta)$ for the argument the False
(3) the True for the argument $\grave{\eta}\Lambda(\eta)$

(4) the False for the argument $\grave{\eta}M(\eta)$

(5) ξ for every other argument ξ

It is easily seen that '$X(\grave{\eta}\Phi(\eta)=X(\grave{\alpha}\Psi(\alpha)$' is the same as '$-\overset{a}{\smile}-\Phi(a)=\Psi(a)$'. Thus, for the objects with a name of the form '$X(\grave{\eta}\Phi(\eta))$', including the True and the False, the same identity criterion holds as for value ranges. Therefore it is possible to identify the truth values as value ranges without inconsistency with axiom V.

This is not to say that it is impossible to stipulate that the truth values are <u>not</u> identical with any of the value ranges. In that case the value of the function $\xi=\zeta$ is always the False when one of the arguments is a truth value and the other a value range. One might think that Frege did not choose this possibility because he was guided by one of his methodological principles, viz., coherence. But it is more plausible that Frege rejected this second possibility because in the first one the indeterminacy - <u>Unbestimmtheit</u> - of his value ranges was reduced. In par. 10 he started from the principle that, given a choice of value ranges $\grave{\varepsilon}\Phi(\varepsilon)$, even objects with a name of the form '$X(\grave{\varepsilon}\Phi(\varepsilon))$' - where $X(\xi)$ is a function which never has the same value for different arguments - satisfy the identity condition of axiom V:

$$\vdash (X(\grave{\varepsilon}\Phi(\varepsilon))=X(\grave{\alpha}\Psi(\alpha))=(-\overset{a}{\smile}-\Phi(a)=\Psi(a))$$

By stipulating the True as the value range of the function $-\xi$ the value range of all concepts under which the True and only the True falls is fixed as the True itself; similarly, by stipulating the False as the value range of the function $\xi=(\neg\overset{a}{\smile}-a=a)$, the value range of all concepts under which the False and only the False falls is fixed as the False itself. In this way, the value ranges are determined as far as possible at this stage of the development of Frege's system (1893, p. 18):

> Wir haben hiermit die Werthverläufe so weit bestimmt, als es hier möglich ist.

It is important to see that these stipulations are two out of many possibilities. For example, it would also be consistent with axiom V to stipulate that the True is the value range of a concept under which the False and only the False falls. The identification of the True with the value range of a concept under which only the True falls might suggest itself, because in this way the extension of a concept under which only one certain object falls is identified with that object. But this cannot be taken as a reason for the special choice of the stipulations for the truth values. First of all these stipulations cannot be generalized in the sense that every object can be identified with a value range, namely the extension of a concept under which it falls as the only object. For if we take '$\acute{\alpha}\Phi(\alpha)$' for '$\Delta$' in

$$'\acute{\varepsilon}(\Delta=\varepsilon)=\Delta'$$

then the result

$$'\acute{\varepsilon}(\acute{\alpha}\Phi(\alpha)=\varepsilon)=\acute{\alpha}\Phi(\alpha)'$$

would be the same as

$$'\overset{a}{-\!\smile\!-}(\acute{\alpha}\Phi(\alpha)=a)=\Phi(a)'$$

but this expression only signifies the True when $\Phi(\xi)$ is a concept under which only one object, namely $\acute{\alpha}\Phi(\alpha)$ falls. Because this need not be the case, the stipulation cannot be generally maintained (1893a, p. 18, n. 1):

> Da dies nicht nothwendig ist, so kann unsere Festsetzung in ihrer Algemeinheit nicht aufrecht erhalten bleiben.

Of course, the identification of the True with the value range of the concept $-\xi$ cannot be defended with the argument that this value range is the only object which falls under that concept because the True is the only object which falls under that concept. Admittedly, Frege himself referred to the Leibniz-Boolean logic when he commented on the possibility of transforming a general identity statement - that is an expression of the form ' $-\overset{a}{\smile}-\Phi(a)=\Psi(a)$ ' - into an identity statement between value ranges, but Frege's value ranges cannot be considered as Boolean classes. The idea of identifying the value range of a concept under which only one object falls with that same object is not comparable to the idea of identifying "monadic" or "singular" classes with their only member. Frege left no misunderstanding about the latter idea when he wrote his "Kritische Beleuchtung einiger Punkte in E. Schröders Vorlesungen über die Algebra der Logik" (1895a, p. 445):

> Nun ist unsere Annahme, dass singuläre Klassen mit Individuen zusammenfallen, eine nothwendige Folge der Auffassung, dass die Klassen aus Individuen bestehen, einer Auffassung, die dem Gebietekalkul gemäss ist und diesem entstammt. Wie wir hier sehen, ist diese Auffassung für den logischen Gebrauch ungeeignet, und der Gebietekalkul, weit entfernt für die Logik von Nutzen zu sein, erweist sich auch hier nur als irreleitend.

(According to Frege, the difference between the pure calculus of regions - *der reine Gebietekalkul* - and the domain of logic - *das logische Gebiet* - is that the former does not bring classes in connection with concepts whereas the latter does (1895a, p. 442). The conception of classes as consisting of individuals clearly belongs to the pure calculus, whereas the conception of classes as value ranges of concepts belongs to logic. However, the two conceptions, taken together with the principle that every singular class coincides with its only element, lead to disaster, as Frege showed in the paragraph preceding the above quotation. His demonstration is along the same lines as the above-mentioned examination of $\acute{\varepsilon}(\Delta=\varepsilon)=\Delta$.)

Frege concluded that the interpretation according to which a class *consists* of individuals so that the individual thing coincides with the conclusion that Frege's identification of the True and the False with certain value ranges was one of the possibilities for satisfying the first condition with respect to value ranges. Yet the story is not finished. Frege introduced the first-level function

$$\backslash\xi$$

which replaces each value range of a concept under which only one object falls by that one object, but leaves everything else unchanged. Frege said that we have here a substitute for the definite article of the language, which serves to form proper names from concept words (1893a, p. 19), for example 'die positive Quadratwurzel aus 2' from 'positive Quadratwurzel aus 2'. This seems a nice example of the fact that (ordinary) language and the 'language' of the Grundgesetze are not unrelated. But it also shows that there are differences: assuming that there are negative and irrational numbers, the expression 'die Quadratwurzel aus 2' is ambiguous - zweideutig - and (for that very reason) meaningless - bedeutungslos - (1893a, p. 19); on the other hand, the expression

$$'\backslash\grave{\varepsilon}(\varepsilon^2=1)'$$

designates something - hat eine Bedeutung - assuming that a suitable definition of the sign for the operation of squaring has been given. For this expression denotes the same object as

$$'\grave{\varepsilon}(\varepsilon^2=1)'$$

simply because $\grave{\varepsilon}(\varepsilon^2=1)$ is not a value range of a concept under which only one object falls. The situation is clear: (natural) languages have the defect that expressions are possible within them, which - according to their grammatical form - seem destined to designate an object, but nevertheless do not reach their destination (1892a, p. 40). On the other hand: it is required of a logically perfect language - Begriffsschrift - that every expression construed as a proper name in a grammatically correct way out of introduced signs, in fact designates an object, and that no sign is introduced as a proper name without assuring that it designates something (1892a, p. 41). For Frege, logical analysis of language does not include that the "logical imperfections" of language are taken over. (We know this already from the discussion in the first section of this chapter.) Thus, it comes as no surprise that there are discrepancies between Frege's formal system and - for example - the German language. This leads to the question whether such discrepancies are responsible for a breakdown of Frege's programme when applied to the language of ordinary discourse. (It can be argued that the formulation of this question is too general. In _Grundgesetze_, Frege tried only to establish a system of logic for ordinary arithmetical discourse. But let us grant that Frege reckoned from the beginning with the possibility of extending the system to other discourses.)

Bell (1979a) gives no examples in connection with the aforementioned function $\backslash\xi$. As a matter of fact, Frege used this function when he introduced the crucial function with two arguments $\xi\frown\zeta$ which coincides in standard cases with the relation of membership between an object ξ and a value range ζ.

A simple application of the function $\backslash\xi$ is, for example, $2=\backslash\acute{\varepsilon}(\varepsilon+3=5)$, which is (the) True, but we have also $\acute{\varepsilon}(\varepsilon+3)=\backslash\acute{\varepsilon}(\varepsilon+3)$, which is the True too, because the function $\backslash\xi+3$ is not a concept. The last example seems hardly relevant for the description of arithmetical discourse.

Bell's examples all have to do with Frege's second condition for the admissibility of functions in general. Take the functions

$$—\xi \quad \text{and} \quad \begin{array}{l} —\!\!\!\top\!\!—\ \xi \\ \quad\ \llcorner\!—\ \zeta \end{array}$$

On the ground of the given stipulations we have, for example (cf. Frege 1891a, p. 21):

$-\!\!-4$ is the False

and (cf. Bell 1979a, p. 21):

$$\begin{array}{l} -\!\!\!-\!\!\!\top\!\!-\ \text{Julius Caesar} \\ \quad\ \llcorner\!\!-\ \text{The Eiffel Tower is the True.} \end{array}$$

But this does not disturb Bell; he suggests that the first function has the concept "is identical to the True" as its ordinary language equivalent. The second function is interpreted in a similar way. According to Bell, the informal counterpart of the last formula then is:

> 'If the Eiffeltower is identical to the True,
> then Julius Caesar is identical to the True'.

Bell is right in so far as the functions $-\!\!-\xi$ and $\xi = -\!\!\overset{a}{\smile}\!\!-\, a = a$ always have the same value for the same argument, but I fail to see that the expression 'is identical to the True' belongs to ordinary language. A simple solution seems to be that those "deviations" come under the heading of the "don't cares" from the side of the ordinary language. But this Quinean turn seems somewhat anachronistic in connection with Frege. It is also not necessary to explain away these (presumed) incongruencies between a natural language and a logically perfect language. Frege could simply not take over the "concept-like formations" of an ordinary language literally, because these are (often) multi-interpretable, making them unsuitable for logical uses (1903a, p. 69):

> Solche begriffsartige Bildungen kann die Logik nicht als Begriffe anerkennen; es ist unmöglich, von ihnen genaue Gesetze aufzustellen. Das Gesetz des ausgeschlossenen Dritten ist ja eigentlich nur in anderer Form die Forderung, dass der

> Begriff scharf begrenzt sei. Ein beliebiger Gegenstand Δ fällt entweder unter den Begriff Φ, oder er fällt nicht unter ihn: tertium non datur.

The principle of the excluded middle can be (and has been) interpreted in several ways; one of the interpretations says that it is connected with a form of Platonism in the sense that, for example, numbers "exist independently" of a thinking subject. This seems incompatible with Frege's characterization of the principle as a demand - Forderung - , especially if one bears in mind that in order to fulfil this demand one cannot always appeal to the usual significations, because these are often lacking. In such cases, one has to make stipulations - Festsetzungen - how newly introduced symbols are to be dealt with. Suppose that one wants to introduce the symbol '⊙' for the Sun in that stage of the construction of the logical system in which the value ranges have just been introduced. Then the only decision that has to be made is whether the Sun is or is not identical with the value range of an available concept. The situation is not principally different from the one in which stipulations are given for the truth values. However, some remarks in the second volume of Grundgesetze make the issue more complicated, because Frege does not always consider it in the light of the step by step construction of the logical system. As a matter of fact he opposed the usual practice in mathematics of "piecemeal defining" (1903a, p. 70):

> Hieraus folgt nun die Unzulässigkeit des in der Mathematik so beliebten stückweisen Definirens. Dies besteht darin, dass man die Definition für einen besonderen Fall giebt - z. B. für den der positiven ganzen Zahlen - und von ihr Gebrauch macht, dann nach manchen Lehrsätzen eine zweite Erklärung folgen lässt für einen anderen Fall - z. B. für den der negativen ganzen Zahlen und der Null - wobei dann oft noch der Fehler gemacht wird, für den schon erledigten Fall noch einmal Bestimmungen zu treffen.

Instead, Frege required "complete definitions" - *vollständige Definitionen* - (1903a, p. 69), not only for concepts, but for all sorts of functions. For these yield concepts when they are partially saturated (1903a, p. 74):

> Wenn z. B. die Beziehung des Grösserseins nicht vollständig definirt ist, so ist man auch nicht sicher, ob eine durch theilweise Sättigung daraus gewonnene begriffsartige Bildung, wie *grösser als Null* oder *positiv* ein eigentlicher Begriff sei.

And then one learns how broad the requirement of completeness is - in the sense that a definition of a concept unambiguously determines for *each* object whether or not it falls under that concept:

> Dazu müsse z. B. auch bestimmt sein ob der Mond grösser als Null sei.

If the moon presents only one example, then there seems little room for constructivism. However, one has to take into account Frege's first condition (cf. p. 39). Then the symbol 'α' denotes an object only if we have an identity criterion or a means of recognition which provides a signification - here a truth value - for every expression of the form '$\xi = \alpha$'. When this is available, then Frege's approach also assures that a truth value is provided for the formal counterparts of sentences about the moon such as 'the moon is greater than zero'. We can expect this sentence to be false if the moon is stipulated as not identical with any value range; *mutatis mutandis* the same holds for a sentence like 'Caesar is a prime number'. Formally there is nothing wrong with this approach. It is true that one can find it absurd that, for example '(5=2+4) is green' becomes a false sentence instead of "nonsensical", but how can one defend that position? Under the influence of an Aristotelian tradition, one could point out that planets belong to a different ontological category than numbers, or that truth values must differ ontologically from coloured objects. But in doing so, one must not burden Frege with a (wrong) ontological position. Precisely the fact that Frege assigned a truth value to every sentence composed of significant expressions suggests that he did not mean it as an ontological position. For Frege, truth values, planets and numbers belong to the same logical category. His requirements, which resulted in providing a truth value for alleged nonsensical expressions, were logical requirements.

To sum up: on the one hand, I claim to have shown that Frege's position can be explained by referring to his logical requirements. On the other hand, those who want to defend the position that an expression such as '(5=2+4) is green' has to be considered nonsensical, should advance a convincing argument that their position is ontologically neutral.

Bell (1979a) gives an argument that the alleged nonsensical expressions cannot denote a truth value because they cannot have a sense (Sinn). In the next section, questions concerning Frege's distinction between "sense" and denotation (Bedeutung) will be dealt with. It is in this context that Frege's problematic comment that judging comes down to "the discerning of parts within the truth value" can be discussed.

On the supposed ontological commitments in "Ueber Sinn und Bedeutung"

Though Frege himself called the introduction of the notation for value ranges "one of the most successful completions" of his Begriffsschrift (1893a, p. 16), publications in the period of the Frege-revival after Carnap's Meaning and necessity (1947a) made it appear as if his distinction between sense - Sinn - and denotation - Bedeutung - was the most important innovation in his second system. Eventually some philosophers were inclined to consider Frege primarily a philosopher of language, who had presented a theory of meaning as part of a general account of the workings of language. It cannot be denied that philosophical logic has a connection with such an account, for philosophical logic aims at a logic which serves as a model for the logical behaviour of expressions of a natural language. But this does not imply that a philosophical logician aims at a theoretical understanding of other, non-logical functions of a natural language.

When talking about phenomena outside the field of logic, Frege confined himself mostly to general remarks. Only his discussions of sense and denotation form an exception. This poses a problem that can be formulated as follows: Is Frege's view - that an expression for an object in the widest sense expresses a certain "sense" - concerned with extra-logical phenomena, or does the distinction between "sense" and "denotation" have to be seen as a logical distinction? The latter position does not find support in Frege's technical works: in both Function und Begriff and Grundgesetze der Arithmetik, the question of the sense of an expression as distinct from its denotation is scarcely touched upon. It arises in connection with different names of the same truth value, such as '$2^2=4$', '$2>1$', '$4.4=4^2$' and '$2+2=4$'. One is tempted to infer that the only reason why Frege talked about senses here was his wish to meet the objection that for example '$2^2=4$' and '$2>1$' "say something different, express different thoughts" (1891a, p. 13). This seems to be neglected in a correct equation such as '$(2^2=4)=(2>1)$'.

The notions of sense and thought - Frege called the "sense" of a name of a truth value a thought - are introduced in a way that may appear arbitrary and artificial, as Frege himself admitted (1891a, p. 14). He referred to the essay "Ueber Sinn und Bedeutung" for a further justification - nähere Begründung - . Moreover, Frege did not give a notation for senses, a peculiarity that has to be cleared up: in order to denote a truth value we can write down a sentence, but how do we denote a sense? The question does not occur in Grundgesetze and one is tempted to conclude that senses do not have a direct logical significance at all, or that Frege's notions of sense and denotation can not be treated as if they stand on the same level. However, this conclusion seems to contradict one of the most conspicuous contributions of "Ueber Sinn und Bedeutung". Didn't Frege here apply the distinction to the logical treatment of several types of compound sentences with a dependent clause? In the sentence 'Copernicus glaubte, dass die Bahnen der Planeten Kreise seien', the expression 'dass die Bahnen der Planeten der Kreise seien' denotes the sense - Sinn - of the words 'die Bahnen der Planeten sind Kreise'. This served to explain why the replacement of an expression within a dependent clause by another with the same denotation does not provide us with a correct deduction; to achieve this we can only replace an expression by another with the same sense. From this example, we learn that thoughts can be denoted and that they are objects from a logical point of view. But do they form an ontological kind? Some remarks on the nature of judgment in "Ueber Sinn und Bedeutung" seem to indicate that they do, for example (1892a, p. 34):

> Aber soviel möchte doch schon hier klar sein, dass in jedem Urtheile - und sei es noch so selbstverständlich - schon der Schritt von der Stufe der Gedanken zur Stufe der Bedeutungen (des Objectiven) geschehen ist.

In my opinion, the difference in level between thoughts and truth values can be explained in such a way that there is no need to resort to ontological levels when interpreting this passage. I believe that Frege's text gives evidence for an interpretation in exclusively "linguistic" and epistemic terms. That thoughts are language-dependent in a way that truth values are not, was stated by Frege in a straightforward answer to the question how we can talk about senses (1892a, p. 28):

> Wenn man von dem Sinne eines Ausdrucks 'A' reden will so kann man dies einfach durch die Wendung "der Sinn des Ausdrucks 'A'".

Whereas one can use a sentence in order to denote a truth value, the simplest way of denoting a thought makes use of a quotation of a sentence, or in other words, mentions a sentence. Apparently the sense of an expression depends on the medium in which the expression is formulated, in a way in which the denotation of the expression, if there is any, is not. But this does not imply that senses are "subjective" (Frege 1892a, p. 30). Knowledge of a linguistic medium is not a subjective affair (Frege 1892a, p. 27):

> Der Sinn eines Eigennamens wird von jedem erfasst, der die Sprache oder das Ganze von Bezeichnungen hinreichend kennt, der er angehört; damit ist die Bedeutung aber, falls sie vorhanden ist, doch immer nur einseitig beleuchtet.

Frege even used the term 'objective' in his comparison of a sense with the real image which is projected by a lens into the interior of a telescope (1892a, p. 30):

> Das Bild im Fernrohre ist zwar nur einseitig; es ist abhängig vom Standorte; aber es ist doch objectiv, insofern es mehreren Beobachtern dienen kann.

In the case of the telescope, different positions towards the same object can yield different images. Similarly in the case of language, different formulations denoting the same object can give different "senses". If this is a serious comparison, then it is not clear why Frege added the qualification "objective" - <u>(Stufe) des Objectiven</u> - to the "level of denotations". But let me postpone questions of objectivity until the next section. There are other difficulties in the text of "Ueber Sinn und Bedeutung". Do two syntactically different designations of the same object always express different senses [14]? Frege's well-known statement (1892a, p. 26) that a difference can only come about if the difference of the sign corresponds to a difference in the way in which the designated object is given - <u>in der Art des Gegebenseins</u> - does not help much. Take the example of the straight lines a, b, c, connecting the vertices of a triangle with the midpoints of the opposite sides: no doubt the designations 'Der Schnittpunkt von a und b' und 'Der Schnittpunkt von b und c' express different senses, but does the same apply to the designations 'Der Schnittpunkt von a und b' und 'Der Schnittpunkt von b und a'? The fact that this example was discussed by Frege in the context of the "epistemic value" - Erkenntnisswerth - of identity statements, makes the matter even more complicated (1892a, p. 26):

> Wenn sich das Zeichen "a" von dem Zeichen "b" nur als Gegenstand (hier durch die Gestalt) unterscheidet, nicht als Zeichen; dass soll heissen: nicht in der Weise, wie es etwas bezeichnet: so würde der Erkenntnisswerth von $a=a$ wesentlich gleich dem von $a=b$ sein, falls $a=b$ wahr ist. Eine Verschiedenheit kann nur dadurch zu Stande kommen, dass der Unterschied des Zeichens einem Unterschiede in der Art des Gegebenseins des Bezeichneten entspricht.

Though Frege sometimes spoke as if the problem of analyticity in a natural language had been solved avant la lettre, he was certainly aware of principal differences between the requirements of a logically perfect language and the actual situation in natural languages (1892a, p. 27-28):

> Gewiss sollte in einem vollkommenen Ganzen von Zeichen jedem Ausdrucke ein bestimmter Sinn entsprechen; aber die Volkssprachen erfüllen diese Forderung vielfach nicht, und man muss zufrieden sein, wenn nur in demselben Zusammenhange dasselbe Wort immer denselben Sinn hat.

Given the problems connected with the application of Frege's distinctions to ordinary language, it seems wise to confine the discussion of the difference in level between thoughts and truth values to the field of his formal system as it is presented in Grundgesetze der Arithmetik. It is to be expected that the sense of a formula of this system is unambiguously determined in a certain way. How is this to be understood? An answer is given in section 32 of Grundgesetze der Arithmetik, 1. Band, following a demonstration that primitive names of the second system and all names correctly composed of them, signify something (1893a, p. 50):

> Aber nicht nur eine Bedeutung, sondern auch ein Sinn kommt allen rechtmässig aus unsern Zeichen gebildeten Namen zu. Jeder solche Name eines Wahrheitswerthes <u>drückt</u> einen Sinn, einen <u>Gedanken aus</u>. Durch unsere Festsetzungen ist nämlich bestimmt, unter welchen Bedingungen er das Wahre bedeute. Der Sinn dieses Namens, der <u>Gedanke</u> ist der, dass diese Bedingungen erfüllt sind.

It follows that the sense of a name of a truth value can be stated in the informal (meta)language of Frege's second system. Indeed, at several places in <u>Grundgesetze</u>, Frege explained how an expression could be rendered in words - <u>in Worten wiedergegeben</u> - . At one place, he made explicitly clear how the sense of the name of a truth value could be better recognized by turning it into another expression: "Um den Sinn hiervon besser zu erkennen, verwandeln wir es durch Wendung" (1983a, p. 71). He thereby [15] kept his promise of section 32 of <u>Grundgesetze I</u>, where he had stated (1893a, p. 51):

> Es wird die Aufgabe des Lesers sein, sich den Gedanken jedes vorkommenden Begriffsschriftsatzes klar zu machen, und ich werde mich bemühen, dies im Anfange möglichst zu erleichtern.

Consequently, the sense of a formula of Frege's second system can be understood as soon as one knows the elucidations, stipulations and definitions underlying the notational system. Because these are explicitly stated, theoretical problems in understanding the "thoughts" expressed by sentences of this system cannot arise, provided that Frege's informal elucidations of the logically primitive signs had the intended effect. The question of the difference in epistemic level between thoughts and truth values can now be explained by the above account: one can know what is expressed by a formula on the basis of the explicit arrangements for the system, without knowing whether the formula is true or false [16].

This difference in level between thoughts and truth values is not an ontological difference. But my explanation seems to fit only Frege's exposition in <u>Grundgesetze der Arithmetik I</u> and does not account for some peculiar remarks in "Ueber Sinn und Bedeutung". Here Frege commented on the above-mentioned difference in level between thoughts and denotations in general - not only truth values. He did this with formulations which can create the impression that he left the platform of logical, linguistic or epistemic discussions for an elaboration on an ontological level.

First of all there is the crucial quotation in paragraph 19 (1892a, p. 34-35):

> Ein Wahrheitswerth kann nicht Theil eines Gedankens sein, sowenig wie etwa die Sonne, weil er kein Sinn ist, sondern ein Gegenstand.

Second, we have the notorious comments on judging in paragraph 21 (1892a, p. 35-36):

> Urtheilen kann als Fortschreiten von einem Gedanken zu seinem Wahrheitswerthe gefasst werden. Freilich soll dies keine Definition sein. Das Urtheilen ist eben etwas ganz Eigenartiges und Unvergleichliches. Man könnte auch sagen Urtheilen sei Unterscheiden von Theilen innerhalb des Wahrheitswerthes. Diese Unterscheidung geschieht durch Rückgang zum Gedanken. Jeder Sinn, der zu einem Wahrheitswerthe gehört, würde einer eignen Weise der Zerlegung entsprechen. Das Wort "Theil" habe ich hier allerdings in besondrer Weise gebraucht. Ich habe nämlich das Verhältniss des Ganzen und des Theils vom Satze auf seine Bedeutung übertragen, indem ich die Bedeutung eines Wortes Theil der Bedeutung des Satzes genannt habe, wenn das Wort

> selbst Theil dieses Satzes ist, eine Redeweise, die freilich anfechtbar ist, weil bei der Bedeutung durch das Ganze und einen Theil der andere nicht bestimmt ist, und weil man bei Körpern das Wort Theil schon in anderm Sinne gebraucht. Es müsste ein eigner Ausdruck hierfür geschaffen werden.

The crucial expression in the quotation is the term 'Theil'. As we shall see, Frege returned to a reflection on uses of this term in later discussions, especially in his "Schriftstück" for Ludwig Darmstaedter (Frege 1969a, p. 273-277).

In the first quotation it is said that the sun is not part of a thought. Taken literally, it cannot be maintained that this is the case for logical, linguistic or epistemic reasons. The sun is not part of an actual thought because the world is that way. This can be called an ontological reason. It seems to follow that the statement that a truth value cannot be part of a thought is also based on, or presupposes, an ontological position: "there is in the world a realm of denotations - including the truth values as well as the sun - and a different realm of senses - including thoughts". However, the situation is not that simple. In the first quotation, the word 'Theil' is used in connection with thoughts, whereas in the second quotation the part-whole relation is transferred to denotations. Frege said explicitly that in the latter case the word 'Theil' is used in a different sense than in connection with bodies. That is, one can speak as if the sun is part of a truth value without being troubled by analogies in the "real" world. Moreover, what would be the analogue of a truth value here? The purpose of speaking about "parts within" a truth value seems to be a further elucidation of Frege's special views about "judging". Frege had to make clear in which sense he considered judging not merely the apprehending of a "thought", but also the acknowledging of its truth. How could he explain that in a judgment, the two levels of thoughts and truth values were involved? The statement that judging may be viewed as an "advancement" - <u>Fortschreiten</u> - from a thought to its truth value doesn't elucidate much. But if one starts from two different judgments, say that '3>2' denotes the True and that '4>3' denotes the True, one can explain the difference by pointing to the different ways in which the two sentences are composed. That the sentences have a different composition can be made clear in Frege's terminology as follows: the sentence '3 >2' can be split up into two parts, an argument expression, '3' for example, and a function expression, 'ξ>2' in this case, whereas the sentence '4 >3' shows a different function expression, '4> ξ', for the same argument expression (cf. Frege 1891a, p. 27.)

The relation between a truth value and a thought which belongs to it in a given judgment can now be elucidated by saying that the thought corresponds to a particular "splitting" of the truth value into parts, of which at least one is a function. (That each part can be a function is clear from the simple fact that for example $-\overset{\mathrm{a}}{\smile}-\mathrm{a}=\mathrm{a}$ is the True; the parts here are the second-level function $-\overset{\mathrm{a}}{\smile}-f(\mathrm{a})$ and the first-level function $\xi=\xi$.) In this sense judging could be said to be a discriminating of parts - such as objects and functions - within the truth value. Then, instead of saying that judging can be viewed as an advancement from a thought to its truth value, we can express it the other way round, namely as a going back - <u>Rückgang</u> - from a truth value to a thought. At the same time we have gained insight into how Frege could talk about senses - <u>Sinne</u> - in this connection:

> Jeder Sinn, der zu einem Wahrheitswerthe gehört, würde einer eignen Weise der Zerlegung entsprechen.

Nevertheless there is a problem which can be illustrated by the above example. The sentence '3>2' could be split into '3' and 'ξ>2'. But one can go further by splitting the last expression again (1891a, p. 27-28):

> Wir können den ungesättigten Theil ≫x > 2≪ weiter in derselben Weise zerlegen in ≫ 2 ≪ und ≫x > y≪ , wo nun ≫y≪ die leere Stelle kenntlich macht, welche vorher durch ≫2≪ ausgefüllt war. Wir haben in

$$x>y$$

> eine Function mit zwei Argumenten, deren eines durch »x« , deren anderes durch »y« angedeutet ist, und in
>
> $$3>2$$
>
> haben wir den Werth dieser Function für die Argumente 3 und 2.

There has been talk of two different splittings of '3>2', one into '3' and '$\xi>2$', another into '3', '2' and '$\xi>\eta$'. Does it follow that there are two different senses - Sinne - , each corresponding to one of these splittings? Or is the first sense the same as the second because it contains a part, namely the sense of the expression '$\xi>2$', corresponding to the splitting into '2' and '$\xi>\eta$' made explicit in the second splitting? Frege did not answer such questions, but he did distinguish parts of thoughts. He wrote in Grundgesetze I (1893a, p. 51):

> Die einfachen oder selbst schon zusammengesetzten Namen nun, aus denen der Name eines Wahrheitswerthes besteht, tragen dazu bei, den Gedanken auszudrücken, und dieser Beitrag des einzelnen ist sein Sinn. Wenn ein Name Theil des Namens eines Wahrheitswerthes ist, so ist der Sinn jenes Namens Theil des Gedankens, den dieser ausdrückt.

So much is clear: a truth value itself cannot be a "contribution" to the thought which is expressed by one of its names. The sense of an expression - a thought or "part" of a thought - is different from its denotation - an object - ; consequently we can say that a truth value cannot be "part" of a "thought" any more than (say) the sun can, because it is not a sense - Sinn - but an "object" - Gegenstand - (from a logical point of view).

It is important to see that the term 'part' - 'Theil' - has been used in three different ways:

(1) literally, for the relation between a name - either an object name or a function name - and the sentence of which it is part,

(2) metaphorically, for the relation between the denotation of a name and the denotation of a sentence, if the word is part of the sentence,

(3) metaphorically again, for the relation between the sense of a name and the sense of a sentence, if the name is part of the sentence and contributes in some way to the thought expressed by the sentence.

The literal use of the word 'part' is of course not restricted to linguistic entities; it can also be said for example, that a piece of (solidified) lava is part of Mount Etna. Now it is true that Frege sometimes confused the second and the just-mentioned use when he explained the difference between sense and denotation. But as soon as one realizes that he did this for explanatory reasons, the inference that Frege "ontologized" his logical distinctions seems far-fetched. There is nothing disturbing about the famous passage in a letter of Frege to Jourdain, written in 1914 in which he considered the sentence 'Der Aetna ist höher als der Vesuv', in order to show that the sense of a name is not indispensable in logic. His argument was (1) that the name 'Aetna' can only contribute to the expression of the sense of the sentence by corresponding to a part of this sense, and (2) that this part cannot be the denotation of the name 'Aetna' (Frege 1976a, p. 127):

> Nun, dieser Theil des Gedankens, der dem Namen "Aetna" entspricht, kann nicht der Berg Aetna selbst sein, kann nicht die Bedeutung dieses Namens sein. Dann wäre ja auch jedes einzelne Stück erstarrter Lava, das ein Theil des Aetna ist, auch Theil des Gedankens, dass der Aetna höher ist als der Vesuv. Er scheint mir aber ungereimt, dass Stücke Lava und zwar auch solche, von denen ich keine Kenntnis habe, Theile eines Gedankens sein sollen.

Thus, Frege could conclude that both the denotation of a name, as that about which something is said, and its sense, as "parts" of the thought, are needed.

Frege kept explaining why a truth value cannot be part of a thought, as can be seen from later writings. In so far as his remarks in the manuscript "Meine grundlegenden logischen Einsichten" and his "Schriftstück" for Ludwig Darmstaedter do not present anything new, they can be quoted without comment. For example, a discussion of the expressions "Dass es wahr ist, dass das Meerwasser salzig ist' and 'dass das Meerwasser salzig ist" in the first essay leads to the remark (Frege 1969a, p. 271):

> Dass Wort "wahr" liefert also durch seinen Sinn keinen wesentlichen Beitrag zum Gedanken.

Similarly, Frege wrote in the second essay (1969a, p. 273):

> Die Wahrheit ist nicht Theil des Gedankens. Mann kann einen Gedanken fassen, ohne damit schon dessen Wahrheit anzuerkennen, d.h. zu urtheilen.

But other remarks do not merely echo the formulations of "Ueber Sinn und Bedeutung". Bell quoted especially from "Logische Untersuchungen", where Frege acknowledged a third realm of thoughts (1918a, p. 69):

> So scheint das Ergebnis zu sein: Die Gedanken sind weder Dinge der Aussenwelt, noch Vorstellungen. Ein drittes Reich muss anerkannt werden.

But having posited this "third realm" whose denizens are Thoughts, Frege - according to Bell - must allow the possibility of human contact with it - otherwise thinking would be impossible (1979a, p. 109):

> He is led, therefore, to posit the existence of a special faculty whose function is the contemplation of eternal truths and falsehoods.

For Bell, doctrines such as these represent "the tip of an ugly ontological iceberg". He finds the later essays of Frege hallmarked by an overwillingness "to indulge in the hypostatization of 'realms', 'spheres', and 'worlds'", though he did admit that "much of this ontologizing is, indeed, harmless and can be easily translated into more acceptable terms". But, and this is crucial for the question whether Frege can sooner or later be accused of contaminating logical issues at least with ontological ones, Bell argued that such a translation is impossible for Frege's doctrine in the "Logische Untersuchungen" that the realms of reference and sense are disjoint, for the simple reason that one can refer to a sense; moreover, given Frege's way of distinguishing functions and objects, thoughts are objects. This means, according to Bell, that Frege was wrong in using the distinction between sense and reference in order to mark out two ontological realms (cf. Bell 1979a, p. 109-110).

However, nowhere in Frege's "Logische Untersuchungen" is a realm of "references" - <u>ein Reich der Bedeutungen</u> - contrasted with a realm of "senses" or thoughts. Indeed, Frege's point was merely that thoughts are neither things of the external world - <u>Dinge der Aussenwelt</u> - nor presentations - <u>Vorstellungen</u> - . Of course, thoughts are objects from a logical point of view, just as persons, places, times, numbers, presentations and so on. The parallel goes even further: as it is not for logical reasons that the denotation of the expression 'Copernicus' is a person, it is also not for logical reasons that the expression 'dass die Bahnen der Planeten Kreise seien' (taken from the sentence 'Copernicus glaubte dass die Bahnen der Planeten Kreise seien') denotes a thought. There is nothing problematic in ascribing to Frege ontological views; we can grant that his ontology consisted of categories, comprising things of the external world, presentations and thoughts, together with commands, requests and questions for that matter (cf. Frege 1892a, p. 38). But we can at the same time maintain that the distinction between sense and reference is not a logical or semantic distiction! In that case, one cannot accuse Frege of conflating semantic and ontological status. Frege acknowledged senses, "even" when logic does not apply, as in the case of sentences without a denotation (truth value) [17].

An obvious objection to the view that the distinction between sense and reference is not a logical or semantical distinction, is that Frege himself applied the distinction to the logical treatment of declarative sentences with a dependent clause: didn't he use it to explain why the replacement of an expression within a dependent clause by another which in "normal" contexts (direct discourse) has the same denotation, can change the truth value of the whole sentence? However, the aim of such explanations in "Ueber Sinn und Bedeutung" was not a logic of language games with compound sentences containing some or another kind of dependent clause. The purpose of all the examples on the last ten pages of that article is explicitly stated as being the examination of the conjecture that the truth value of a sentence is its denotation (1892a, p. 36). At the end of the article, Frege could indeed conclude that instances in which a clause is not replaceable by another of the same truth value do not disprove his view that the denotation of a sentence is its truth value (1892a, p. 49-50).

His argumentation rested mainly on the principle that the truth (or falsity) of a given sentence remains unchanged if a constituent of the sentence is replaced by an expression with the same denotation. In "Ueber Sinn und Bedeutung", Frege took it for granted that the denotation of a "proper name" in a natural language, such as 'der Schnittpunkt von *a* und *b*', is the object which we denote with it (1892a, p. 30). But we know that he required the presence of an identity criterion for the admissibility of such object names in a logically perfect language in which rigorous proofs can be given. In "Ueber Begriff und Gegenstand", Frege took it for granted that every expression within a sentence other than a proper name is a concept word, but he restricted his discussion to those concept words which occur as predicate. We know that he also required a criterion for the admissibility of such concept words in a logically perfect language. As we have seen, this was necessary for his treatment of arithmetic and could be reached with the help of special stipulations. But in "Ueber Sinn und Bedeutung", he tried to show that his principle was generally valid for a natural language. This forced him to consider a great number of complex sentences, varying from 'Copernicus glaubte, dass die Bahnen der Planeten Kreise seien' to 'Wenn Eisen specifisch leichter als Wasser wäre, so würde es auf dem Wasser schwimmen'. His strategy had two parts: first, he "interpreted" a dependent clause such as "dass die Bahnen der Planeten Kreise seien' as a proper name of a thought; second, he argued that complex sentences can express more than one thought. In these cases, he tried to make plausible that a dependent clause does not express a thought but only part of a thought (1892a, p. 46). However, nowhere in his discussion did he give more than an informal "logical analysis". The last ten pages of "Ueber Sinn und Bedeutung" are written in the vein of the older tradition of logic text books of, say Sigwart, rather than in accordance with the rigorous approach of Grundgesetze der Arithmetik. Begriffsschriftlich abgeleitet von G. Frege. From these considerations I believe that Frege's line of thought in "Ueber Sinn und Bedeutung" has no ontological implications for his formal system. Frege's contributions in "Ueber Sinn und Bedeutung" are simply incomparable with the approach in Grundgesetze [18].

On Frege's objectivity thesis and its supposed connection with ontology in logical theory

Writing a magnum opus such as Grundgesetze der Arithmetik leaves little time for philosophical digressions. Yet Frege here and there entered the domain of philosophy, in particular when he came to speak about "objectivity". That we have a wide field for research here is shown by recent discussions between Currie, Dummett, Resnik, and Sluga: questions about the origin, nature, and extent of Frege's philosophy of objectivity are not easy to answer; they require a thorough study, not only of Frege's expositions, but also of writings of philosophers who might have influenced Frege. In this section, my concern is with Bell's claim that Frege's objectivity thesis was grounded on an ontological turn in his logical "categories". I shall argue that Bell is wrong, even when Frege meant his objectivity thesis as an expression of an ontological view.

It is not difficult to see why Frege found it necessary to digress philosophically: he wanted to repel the invasion of psychology into mathematics and logic (cf. Frege 1884a, p. VIII). Nor is it strange that he used the objective/subjective terminology: Drobisch did the same in his well-known Neue Darstellung der Logik (Drobisch 1875a, p. 5). We can indeed safely assume that for Frege the distinction between psychological and logical ran parallel with the distinction between "subjective" and "objective": "es ist das Psychologische von dem Logischen, das Subjective von dem Objectiven scharf zu trennen." (Frege 1884a, p. X). But Frege went further than Drobisch by spending a whole section of Die Grundlagen der Artihmetik on his notion of objectivity. As we know, the outcome of this discussion was summarized in the preface of the first volume of Grundgesetze der Arithmetik as: "ich erkenne ein Gebiet des Objectiven, Nichtwirklichen an, während die psychologischen Logiker das Nichtwirkliche ohne weiteres für subjectiv halten." (Frege 1893a, p. XVIII).

This is Frege's objectivity thesis. It can be interpreted in several ways; I will offer three possibilities.

First, Frege's objectivity thesis can be interpreted as the epistemological view that the character of logico-mathematical knowledge is independent of an investigation into the subjective conditions for that knowledge. In that case, we do not need to assume that Frege intended to defend a so-called Platonism - the view that the ideal entities lead an existence independently of the knowing mind; only that he contended that logic and mathematics can be built up independently of any answer to the question what the subjective conditions are for the possibility of mathematical thinking (Beth 1948a, p. 198-199).

Second, Frege's objectivity thesis can be seen as an elaboration of the simple principle that "what is true, is true, independent of our acknowledgement": "Was wahr ist, ist wahr unabhängig von unserer Anerkennung" (Frege 1969a, p. 2 and similarly p. 3). In this view, the content of, say the equation 2+3=5 is not a result of an inner process or a mental activity, but something "objective", that is, what is the same for all beings gifted with intellect (Vernunftwesen), for all who are able to grasp (fassen) it (cf. Frege 1969a, p. 7). In this sense, a number, the North Sea, the earth, the sun, the number of a flower's petals, the colour of a flower, the axis of the earth, the centre of mass of the Solar system are all, what they are, not what they look like or how they appear, in other words objective; though not all of them are actual (wirklich) in the sense that they "act" on our senses or at least produce effects which act in that way. This interpretation acknowledges that Frege went rather deeply into "philosophy" in order to convince psychologistic logicians or empiricist philosophers of arithmetic. But he was realistic enough to realize that not everyone would take philosophical digressions seriously. Why else would he have asked halfway through the preface of Grundgesetze I: "Mathematiker, die sich ungern in die Irrgänge der Philosophie begeben, werden gebeten, hier das Lesen des Vorworts abzubrechen" (Frege 1893a, p. XIV, n. 2).

Third, it can be interpreted as an ontological view: everything "objective" has a being independent of the judging subject: "hat einen vom Urtheilenden unabhängigen Bestand" (cf. Frege 1893a, p. XVIII). This interpretation is in conformity with the way in which, according to Frege's contemporary, Haym, common sense uses the word 'objectivity': "In weiterem Sinne wird aber von dem gesunden Verstande das Wort Objectivität gerade in der Absicht gebraucht, um damit ein von dem erkennenden Subjecte durchaus losgelöstes, und unabhängig für sich bestehendes Dasein zu bezeichnen." (Haym 1883a, p. 7).

However, whatever the "correct" interpretation of Frege's philosophical talk may be, even under the strongest interpretation it does not follow that "Frege sought to protect objectivity in science by grounding his basic categories ontologically, by adopting an extreme form of platonic realism" as Bell thought when he concluded that "not only concrete objects, but abstract objects, concepts, functions, senses, and truth-values are accorded ontological status as a means of protecting their objectivity against the pernicious encroachment of philosophical idealism and scientific psychologism" (Bell 1979a, p. 74).

Nowhere did Frege defend any objectivity thesis on the ground of an ontologization of his logical distinctions; how could he have, when they were only made for the adequate expression of logical laws? (Frege 1969a, p. 5):

> Wie jede Wissenschaft hat auch die Logik ihre Kunstausdrücke, Wörter, welche z.T. auch in der nichtwissenschaftlichen Sprache, aber nich ganz in demselben Sinne gebraucht werden. (...) Umso geeigneter aber ist ein Ganzes von Kunstausdrücken, je kürzer es die gesamte Gesetzmässigkeit genau zum Ausdruck bringen kann.

This was written in the time that Frege still used the expression 'beurtheilbarer Inhalt'; but from the above quotations, it can be seen that already then he was convinced of the objective character of truths. In his second system, logical objects (truth-values and value ranges) were introduced; but again what matters were Frege's logical convictions (logische Ueberzeugungen); this system had also to be judged in the light of the results, such as the proofs of the arithmetical principles in section II of Grundgesetze der Arithmetik. The last paragraph of the preface to volume I of that book gives evidence for this, for example (Frege 1893a, p. XXVI):

> Und nur das würde ich als Widerlegung anerkennen können, wenn

> jemand durch die That zeigte, dass auf andern Grundüberzeugungen ein besseres, haltbareres Gebäude errichtet werden könnte, oder wenn mir jemand nachwiese, dass meine Grundsätze zu offenbar falschen Folgesätzen führten.

The latter was just what came to pass. As we shall see in the following sections, Frege felt urged to re-examine his second logical theory. He did this in a way that Bell's accusations are simply out of place. So let us turn to Frege's reconsiderations of his second system.

CHAPTER THREE

FREGE'S RECONSIDERATIONS OF HIS SECOND SYSTEM

Introduction

It might be concluded from my treatment of Frege's second system in the preceding sections that the technical elaboration of this system yields no problems as long as Frege reckoned with his criteria of definability. However, the situation is not that simple; this already appeared from some questions which Russell put to Frege, not only about the notorious "difficulty" (Schwierigkeit) on the one point of the contradiction, but also on a difficulty with value ranges and classes.

I shall show in the next two sections that Frege's (second) thoughts about these questions are methodological in character and do not support ontological interpretations of Frege's theory.

Value ranges and classes

We have seen that Frege was very careful when he introduced value ranges in Grundgesetze der Arithmetik. That is not to say that there were no problems in the elaboration of the theory. Especially as regards the function with two arguments

$$\xi \frown \zeta$$

it was desirable to see the consequences of the stipulation for the defining expressions. In par. 34, Frege showed the following:

(1) if the ζ-argument is a value range, the value of the function $\xi \frown \zeta$ is the value of the function of which the ζ-argument is the value range for the ξ-argument as argument:
(Theorem 1) $\vdash f(a) = a \frown \grave{\varepsilon} f(\varepsilon)$.

(2) if the ζ-argument is not a value range, the value of the function $\xi \frown \zeta$ is $\grave{\varepsilon}(\neg\, \varepsilon = \varepsilon)$ for every ξ-argument.

It can be asked when (2) is the case. Are not all logical objects, including the truth values, conceived as value ranges of functions? [19] It seems as if Frege considered solely those object expressions as standing for a (single) value range, which are formed from the second-level function expression '$\acute{\varepsilon}\varphi(\varepsilon)$' by taking a (meaningful) name of a first-level function as argument of the corresponding second-level function. But in the (notorious) note 1 of page 18, he stated that "the way in which an object is given, cannot be considered an invariable property" of that object, because the same object can be given in different ways. Moreover, he identified the only objects which are not "given as" value range, with value ranges.

That there is something wrong in Frege's treatment appears from a remark in the same note, namely that the function $\xi \frown \zeta$ has the general property that Δ is the same as $\acute{\varepsilon}(\varepsilon \frown \Delta)$ also in the case that Δ is not "given" as a value range. It is true that if Δ is a value range, say $\acute{\alpha}f(\alpha)$, then $\acute{\varepsilon}(\varepsilon \frown \Delta) = \acute{\varepsilon}(\varepsilon \frown \acute{\alpha}f(\alpha)) = \acute{\varepsilon}f(\varepsilon) = \Delta$; but if Δ is not a value range, then $\acute{\varepsilon}(\varepsilon \frown \Delta) = \acute{\varepsilon}(-\!\!\top\!\!-\varepsilon=\varepsilon)$. Of course also $\acute{\varepsilon}(\varepsilon \frown \acute{\alpha}(-\!\!\top\!\!-\alpha=\alpha)) = \acute{\varepsilon}(-\!\!\top\!\!-\varepsilon=\varepsilon)$, but one cannot conclude from $\overset{a}{-\!\smile\!-} a \frown u = a \frown v$ to $u=v$. This was already remarked by Russell in his letter of 24.7.1902 to Frege. Russell asked Frege for clarification (1976a, p. 221):

> Aus $\overset{a}{-\!\smile\!-} a \frown u = a \frown v$ kann man nur schliessen $u=v$ wenn man schon weiss dass u und v Werthverläufe sind. Es fragt sich aber, wie man dies wissen kann.

Frege's answer is revealing (1976a, p. 225):

> Sie fragen, wie man es wissen könne, dass etwas ein Werthverlauf sei. Allerdings ein schwieriger Punkt. Nun, alle Gegenstände der Arithmetik werden als Werthverläufe eingeführt. Sobald man einen neuen Gegenstand nicht als Werthverlauf in die Betrachtung einführt, muss man zugleich die Frage beantworten, ob er ein Werthverlauf sei, und zwar

> wahrscheinlich immer mit nein, weil man ihn als Werthverlauf einführen würde, wenn er einer wäre.

Frege here states, in so many words, that for his second system:

(1) all objects of arithmetic are introduced as value ranges,
(2) not every object outside arithmetic is necessarily to be introduced as a value range,
(3) there are objects outside arithmetic which are to be introduced as value ranges.

It follows from (3) that value ranges are not special objects, "created" by Frege in order to "create" mathematical objects. He himself stated this in section 146 of the second volume of Grundgesetze: value ranges are nothing other than a generalization of the extensions of concepts of the older logicians. They can be considered "logical objects", but - again on the ground of (3) - not because they are dealt with in arithmetic. The objects of arithmetic are only a special kind of logical objects, which comprises such different objects as classes of military companies, classes of atoms, and Frege's favourite class of all prime numbers. As to these logical objects, Frege's letter of 28.7.1902 to Russell brings us to the heart of philosophical logic. According to Frege, sentences like 'Die Klasse der Primzahlen umfasst unendlich-viele Gegenstände' and 'Die Klasse der Compagnien eines gegebenen Regiments gehören zwölf Compagnien an', say something about classes. Such classes are not "wholes" or "systems" like physical objects which consist of parts, and can not be identified with them. We have to distinguish between, e.g. a chair, the atoms which form - bilden - the chair, and the class of these atoms. Both the chair and the atoms are "material", but the class of atoms is not. In general (1976a, p. 223):

> Ein Ganzes dessen Theile materiell sind, ist selbst materiell; eine Klasse dagegen möchte ich nicht als

physischen, sondern als logischen Gegenstand bezeichnen.

I conclude that Frege's system in Grundgesetze, though directed toward a rigorous treatment of arithmetic, was not a special logical axiom system for arithmetic alone, but a general logical theory, which could also be used in the logical analysis of non-mathematical reasonings, where objects occur which are not value ranges.

As soon as one considers a language game in which it is said, for example, that there are more cats than dogs, one can give an account in terms of classes. This is not Frege's example, but it can help to make plausible that someone raises questions like "what are classes?" or "how do we grasp logical objects?" - wie fassen wir logische Gegenstände? - . Frege did pose the latter question in the above letter to Russell, who had admitted to having difficulties in seeing what a class is, if it does not consist of objects. (Cf. Frege 1976a, p. 221.) Frege's answer was: as extensions of concepts, or more generally, as value ranges of functions - wir fassen sie als Umfänge von Begriffen, oder allgemeiner als Werthverläufe von Functionen - (o.c., p. 223).

It is illuminating to see how Frege actually went to work in his first publication on the subject, Die Grundlagen der Arithmetik. Here number assignments are analyzed as assertions concerning concepts, that is, numbers "belong to" concepts. In the (informally presented) logical theory, the number belonging to a concept F is characterized as the extension of the concept "equinumerous to the concept F". There is nowhere talk of classes. Frege simply assumed the meaning of the expression 'Umfang des Begriffes' to be known (1884a, p. 117). That is (in the terminology of Van Fraassen), Frege characterized the structure of arithmetical discourse by using logical discourse without an intermediate discourse about "classes". He thereby avoided the dangers of a "pictorial account". According to his lecture "Ueber formale Theorien der Arithmetik", Frege deliberately substituted the word 'Menge' by 'Begriff' - more precisely: by 'Begriffsumfang' - because the word 'Menge' suggested an "accumulation" - Anhäufung - of things in space. I conclude that Frege wanted to avoid any realistic interpretation of his terminology, in the sense of a reference to "real space". This is one reason for believing that Frege did not ontologize his logical distinctions.

I am not implying that Frege had no difficulties in incorporating these extensions of concepts into his original framework of Begriffsschrift. He might first have thought that he could confine himself to concepts; in this case the Begriffsschrift system itself would be sufficient for the characterization of arithmetical discourse. But Frege had to meet two objections: first, that a number is an object from a logical (and a grammatical) point of view; second, that concepts can have identical extensions without themselves coinciding (1884a, p. 80). At the time he wrote Die Grundlagen der Arithmetik, Frege still had the opinion that both objections could be met (o.c.). When he said in his third letter to Russell that "he had long resisted acknowledging value ranges and therefore classes", this was, I believe, because he had hoped to characterize arithmetical statements logically within the early framework of Begriffsschrift [19]. Moreover, a system with both "concepts" and "extensions of concepts" needed an (extra) axiom for the relation between them, the later axiom V. And exactly this axiom incurred suspicion after Russell's derivation of a contradiction in the second system. This was a more serious criticism than spotting an error in note 1 of page 18 of Grundgesetze. As we shall see, Frege's reaction is a paradigm of an "ontology-free" treatment of the question how to achieve required innovations and complications on the side of a formal apparatus.

The contradiction

It is rather curious that the discoverer of a contradiction in Frege's second system did not react directly to the rendering of this system in Grundgesetze der Arithmetik when he wrote his famous letter, but questioned a statement of Begriffsschrift, where Frege's first system was presented. One wonders whether Russell really had had more than a quick look at Grundgesetze. Frege did not need to be upset by the question whether "the predicate w which cannot be predicated of itself can be predicated of itself", nor by Russell's answer that one has to conclude that w is not a predicate. And given Russell's wrestling with the notion of class in The principles of mathematics one wonders whether he saw how the contradiction in terms of classes affected Frege's second system. This version of the contradiction was stated by Russell as follows (Frege 1976a, p. 211):

> Ebenso giebt es keine Klasse (als Ganzes) derjenigen Klassen die als Ganze sich selber nicht angehören. Daraus schliesse ich dass unter gewissen Umständen eine definierbare Menge kein Ganzes bildet.

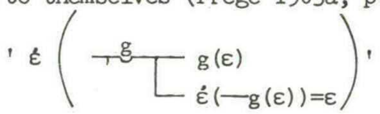
Actually this is not a derivation of a contradiction, let alone a derivation within Frege's second system. This can be seen more clearly from the corresponding section 102 in The principles of mathematics. Here Russell concluded that classes that "as ones" are not members of themselves "as many" do not form a class "as one". He also remarked that the argument cannot show that they do not form a class "as many". The same holds for a version with classes defined by propositional functions; in this case, the conclusion must be that not every propositional function defines a class. However, without such restrictions there is a real contradiction, as Russell showed this in a postscript to his first letter to Frege, by a formalization in Peano's system of the statement 'if w is the class of all x's such that x is not a member of x, then w is a member of w if and only if w is not a member of w.

This means that Russell's informal argument can be reconstrued for Frege's second system too, if one starts from a version which fits this system (Frege 1976a, p. 213-214):

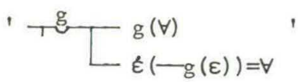
> Uebrigens scheint mir der Ausdruck "Ein Praedicat wird von sich selbst praedicirt" nicht genau zu sein. Ein Praedicat

> ist in der Regel eine Function erster Stufe, die als Argument einen Gegenstand verlangt und also nicht sich selbst als Argument (Subject) haben kann. Ich möchte also lieber sagen: "Ein Begriff wird von seinem eigenen Umfange praedicirt". Wenn die Function $\Phi(\xi)$ ein Begriff ist, so bezeichne ich dessen Umfang (oder die zugehörige Klasse) durch »$\Phi(\varepsilon)$« (die Berechtigung hierzu ist mir nun freilich zweifelhaft geworden). In »$\Phi(\acute{\varepsilon}\Phi(\varepsilon))$« oder »$\acute{\varepsilon}\Phi(\varepsilon) \cap \acute{\varepsilon}\Phi(\varepsilon)$« haben wir dann die Praedicirung des Begriffes $\Phi(\xi)$ von seinem eigenen Umfange.

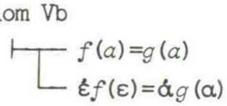
Derivations of the contradiction were presented by Frege himself in the postscript to the second volume of <u>Grundgesetze</u>. One of these derivations starts from a formal representation of the class of classes not belonging to themselves (Frege 1903a, p. 256) [21]:



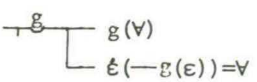
If one uses the symbol ∀ as abbreviation, then the statement that ∀ belongs to itself gets the following representation:



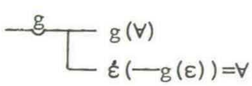
With the help of axiom Vb



and suitable substitutions, both



and



can be derived [22].

A second derivation starts from Theorem 1, and makes use of the following theorems:

(Theorem 77) $$\vdash\!\!\begin{array}{l} F(a\frown\acute{\varepsilon}f(\varepsilon)) \\ \llcorner F(f(a)) \end{array}$$

(Theorem 82) $$\vdash\!\!\begin{array}{l} F(f(a)) \\ \llcorner F(a\frown\varepsilon f(\varepsilon) \end{array}$$

If one takes $'\acute{\varepsilon}(\top\varepsilon\frown\varepsilon)'$ for $'\forall'$ (and for $'a'$ in the just mentioned theorems), then, with the help of suitable substitutions, both

$$\top\acute{\varepsilon}(\top\varepsilon\frown\varepsilon)\frown\acute{\varepsilon}(\top\varepsilon\frown\varepsilon)$$

and

$$-\acute{\varepsilon}(\top\varepsilon\frown\varepsilon)\frown\acute{\varepsilon}(\top\varepsilon\frown\varepsilon)$$

can be derived.

Because Axiom Vb has been used in the derivation of Theorem 1, this theorem also incurs suspicion here: Auf diesen Satz wird also auch hier der Verdacht gelenkt (Frege 1903a, p. 257). This led Frege to a change in his original axiom V; instead of

(Axiom V) $$\vdash\ (\acute{\varepsilon}f(\varepsilon)=\acute{\alpha}g(\alpha))=(\overset{a}{\smile}f(a)=g(a))$$

he postulated [22]

(Axiom V') $$\vdash(\acute{\varepsilon}f(\varepsilon)=\acute{\alpha}g(\alpha))=\overset{a}{\smile}\!\!\begin{array}{l} f(a)=g(a) \\ \llcorner\top\ a=\acute{\varepsilon}f(\varepsilon) \\ \quad\llcorner\top\ a=\acute{\alpha}g(\alpha) \end{array}$$

But before he took this way out, two other adaptations of the second system were investigated.

First Frege discussed Russell's suggestion to forbid formulas of the form

$$'\varphi(\acute{\varepsilon}\varphi(\varepsilon))'$$

This would however run counter to Frege's condition for the admissability of concepts. If $\neg\xi\cap\xi$ is such a concept, then it has to be determined for every object whether it falls under this concept or not. If the value range of this concept, $\acute{\varepsilon}(\neg\varepsilon\cap\varepsilon)$ is admitted at all, then it has to be the case that this value range falls under $\neg\xi\cap\xi$ or does not:

$$\neg\acute{\varepsilon}(\neg\varepsilon\cap\varepsilon)\cap\acute{\varepsilon}(\neg\varepsilon\cap\varepsilon) \quad \text{or} \quad \neg\neg\acute{\varepsilon}(\neg\varepsilon\cap\varepsilon)\cap\acute{\varepsilon}(\neg\varepsilon\cap\varepsilon)$$

This leads to a hypothetical adaptation (Frege 1976a, p. 217):

> Sie wollen, wie es scheint, Formeln wie »$\varphi(\acute{\varepsilon}\varphi(\varepsilon))$« verbieten, um den Widerspruch zu vermeiden. Aber wenn Sie ein Zeichen für den Umfang eines Begriffes (eine Klasse) überhaupt als bedeutungsvollen Eigennamen zulassen, also die Klasse als Gegenstand anerkennen, so muss diese Klasse selbst entweder unter den Begriff fallen oder nicht; tertium non datur. Erkennen Sie die Klasse der Quadratwurzeln aus 2 an, so ist die Frage nicht zu umgehen, ob diese Klasse eine Quadratwurzel aus 2 sei. Sollte sich zeigen, dass diese Frage weder bejaht noch verneint werden könnte, so wäre damit der Eigenname »$\acute{\varepsilon}(\varepsilon^2=2)$« als bedeutungslos erkannt. Oder sollte man die Werthverläufe (Begriffsumfänge, Zahlen) als eine besondere Art von Gegenständen hinstellen, denen gewisse Praedicate weder zu- noch abgesprochen werden können? Das würde doch auch wohl auf grosse Schwierigkeiten stossen.

It is remarkable that Frege mentioned only a methodological objection. This did not change when he returned to this possible rectification in a later letter to Russell (Frege 1976a, p. 227-228) and the same methodological arguments are used in the postscript of the second volume of Grundgesetze (1903a, p. 254-255). Only one remark at the end of the discussion seems to point in another direction:

> Ueberdies kann die Berechtiging uneigentlicher Gegenstände

bezweifelt werden.

However, it seems possible to interpret this remark as a doubt about the justification of a distinction between proper and inproper objects from a logical point of view. What is the difference, if any, in logical form between a statement like 'sechs ist eine Gerade Zahl' and a statement like 'Sirius ist ein Fixstern'? Both, according to Frege, express a subsumption of an object under a concept. With this interpretation, nothing can be inferred about an ontological point of view. Frege was no Meinong.

Frege discussed the hypothetical way out of conceiving class names as improper names, comparable with, say, '̀' in '$\acute{\varepsilon}(\varepsilon=\varepsilon)$'. In that case, class names would not denote anything (1903a, p. 255):

> Sie wären dann anzusehen als Theile von Zeichen, die nur als Ganze eine Bedeutung hätten.

However, this suggestion did not fit Frege's logical intuitions (1903a, p. 255):

> Eine Erklärung des Zeichens ≫2≪ wäre unmöglich; man hätte statt dessen viele Zeichen zu erklären, die als unselbständigen Bestandtheil ≫2≪ enthielten, aber logisch nicht aus ≫2≪ und einem andern Theile zusammengesetzt zu denken wären.

Moreover, there would be no possibility of forming expressions with number variables, and this would exclude general arithmetical theorems. It would also be impossible to speak formally about a number of numbers (o.c.), and there would thus be no adequate characterization of the structure of the arithmetical language game. Frege concluded that his only way out was (1) accepting extensions of concepts, or classes, as (proper) objects, and (2) correcting the axiom which stated a relation between concepts and extensions of concepts.

Some of the consequences of the new axiom V' were already indicated by Frege in his postscript. For example, he did not have to change his stipulations for the True and the False as value ranges. Another case concerns the function $\xi \frown \zeta$. If Γ is a value range, then $\Gamma \frown \Gamma$, that is

$$\acute{\alpha}\left(\;\top\!\!-\!\overset{g}{\smile}\!-\!\!\top\!\!-\!\!\top\!\!-\;\begin{array}{l} g(\Gamma)=\alpha \\ \Gamma=\acute{\varepsilon}g(\varepsilon)\end{array}\right)$$

is the extension of an all-embracing concept, that is, a concept under which every object falls. This affects Frege's treatment of number, but unfortunately Frege confined himself to the following remark (Frege 1903a, p. 264):

> Dies ist wichtig für die Function $\natural\,\xi$. Man könnte zunächst befürchten, dass Begriffe von demselben Umfange nach unsern Festsetzungen dieselbe Anzahl erhalten müssten, obwohl unter den einen ein Gegenstand mehr, als unter den andern, nämlich der Begriffsumfang selbst fiele, sodass man schliesslich nur eine einzige endliche Anzahl erhielte. Indessen kommt bei $\natural\,\acute{\varepsilon}\Phi(\varepsilon)$ nicht der Begriff $\Phi(\xi)$, sondern $—\xi \frown \acute{\varepsilon}\Phi(\varepsilon)$ in Betracht, und unter diesen fällt der Begriffsumfang $\acute{\varepsilon}\Phi(\varepsilon)$ nicht, wenn er auch unter den Begriff $\Phi(\xi)$ fällt.

It is not known how far Frege went in checking the consequences of the new axiom. At least in his postscript he seems not to be troubled that the extension of concepts in the traditional sense was cancelled (1903a, p. 260-261) by his new suggestion for the characteristic property of the second-level function $\acute{\varepsilon}\varphi(\varepsilon)$. This is not so strange, if we remember that Frege's system already contained deviations from traditional conceptions, such as the stipulations for the True and the False. It seems that the difference between the third and the second system for Frege in 1903 was only a matter of complexity (1903a, p. 265):

> Als Urproblem der Arithmetik kann man die Frage ansehen: wie

> fassen wir logischen Gegenstände, insbesondere die Zahlen? Wodurch sind wir berechtigt, die Zahlen als Gegenstände anzuerkennen? Wenn dies Problem auch noch nicht so weit gelöst ist, als ich bei der Abfassung dieses Bandes dachte, so zweifle ich doch nicht daran, dass der Weg zur Lösung gefunden ist.

That Frege's third system also appeared to be inconsistent afterwards, does not make the difference greater.

WHITEHEAD AND RUSSELL

PART TWO

WHITEHEAD AND RUSSELL

Introduction to Part Two

Interpreting and evaluating common-sense views or scientific theories in order to establish a philosophical theory about the world is one of the tasks of philosophy. Traditional metaphysical positions such as phenomenalism, psychical monism, neutral monism, realism and materialism can be seen to have developed in this general way. This is not to say that these positions were easily established - the process of interpreting theories was often a difficult one. More than once, philosophical results were said to be too speculative to be acceptable. Sometimes the whole approach was questioned and philosophers felt forced to ask whether "metaphysics as a science" or "scientific philosophy" was possible.

In the course of the twentieth century, some philosophers argued that the task of interpreting scientific or common-sense theories could be achieved by "rational reconstructions" of such theories. A rational reconstruction of a given theory would make its assumptions explicit, since it had to fulfil two major requirements (Hempel 1952a, p. 11):

> First, the explicative reinterpretation of a term, or - as is often the case - of a set of related terms, must permit us to reformulate, in sentences of a syntactically precise form, at least a large part of what is customarily expressed by means of the terms under consideration. Second, it should be possible to develop, in terms of the reconstructed concepts, a comprehensive, rigorous, and sound theoretical system.

Thus, a rational reconstruction had to be presented as a formal theory, in which the basic axioms and definitions were formalized. Giving such formal reconstructions was considered a first step towards any metaphysics which is to rank as a science. Rational reconstructions serving this aim had to be such as to make the ontological assumptions of theories explicit. That is, they should make two things explicit:

(1) which kinds of objects, which relations or kinds of relations between these objects, which relations or kinds of relations between relations are assumed to exist: the so-called fundamental entities, and
(2) which kinds of objects and relations admit of definition in terms of the fundamental entities: these are "defined entities" which are not assumed to exist; they do not belong to an ontological kind.

Rational reconstructions satisfying these two requirements will be called formal ontological reconstructions. They have to be distinguished from formal reconstructions for which no ontological conclusions are drawn as regards basic or defined notions. The aim of a formal ontological reconstruction of a given theory is to provide it with a formal axiomatization in such a way that the basic assumptions of the axiomatization reflect a definite philosophical position. In a way, intuitions behind this approach go back to the British empiricists, who tried to show that certain "ideas" were derived from other "ideas" (Locke), or "impressions" (Hume). These philosophers, however, did not succeed in making the results of their "derivations" precise. A good illustration of this is Hume's characterization of our conception of time, "which, since it appears not as any primary distinct impression, can plainly be nothing but different ideas, or impressions, or objects disposed in a certain manner, that is, succeeding each other" (Hume, A treatise of human nature, Volume I, 1.2.3). The idea of exact reconstructions was not wholly absent, however; Locke, for example, stated that whereas "the names of simple ideas are not capable of any definitions, the names of all complex ideas are" (Locke, An essay concerning human understanding, 3.4.4).

The nineteenth century Austrian empiricist Ernst Mach anticipated the kind of discipline we are going to discuss in this chapter. In formulating his position that (for us) the world does not consist of enigmatic beings, but that colours, spaces, times, ... are (at least provisionally) "the last elements", he expressed the sort of result ontological reconstructionism would try to establish. But Mach's characterization of matter as "mathematical functional relationships of elements" is still too far removed from the method of formal ontological reconstructions to count as ontological reconstructionism in the above sense. At best, Mach formulated a program, as he himself realized (Mach 1919a, p. 297).

Heinrich Hertz's reconstruction of classical mechanics in terms of space, time and matter reflects a deliberate ontology. Indeed, Hertz's considerations in the introduction to Die Prinzipien der Mechanik are very important in connection with the aims of formal ontological reconstructions. But Hertz did not make use of contemporary results in the development of the axiomatic approach and the theory of logic, unlike Russell and Whitehead, who themselves took active part in this development. Russell can be credited with an early step in the direction of reconstructionism. He conceived the idea of an evaluation of different philosophical theories of time on the basis of formalizations. Subsequently, Russell conceived an ontological foundation for the material world in the last part of The principles of mathematics. Then Whitehead, in his memoir "On mathematical concepts of the material world" of 1906, succeeded in giving formal ontological reconstructionism a clear and definite formulation. He also showed that one and the same scientific theory can have various formal ontological reconstructions which differ with regard to the fundamental entities. This implied that two philosophers can agree about "the facts" without agreeing about the "composition of the world".

Russell, in his book Our knowledge of the external world as a field for scientific method in philosophy of 1914, realized that Whitehead's approach could be extended to a new kind of "scientific philosophy": he propagated a doctrine of formal ontological recontructionism in which a philosopher should not only give a formal ontological reconstruction of theories about the world, but also defend his particular choice of fundamental entities. Thus, he could take advantage of the new analytical techniques within the conception of philosophy advocated by G. E. Moore. It is true that Whitehead already suggested this approach, but in 1906 he was explicitly not "concerned with upholding or combatting any theory of the material world" (Whitehead 1906a, p. 14).

In order to distinguish terminologically between Whitehead's and Russell's position I shall call Whitehead's procedure, in which no philosophical justifications are given, "weak" formal ontological reconstructionism and Russell's doctrine "strong" formal ontological reconstructionism. Only the latter can be considered a kind of "scientific metaphysics": it requires an explication why one has chosen for a certain ontology.

Russell's reconstruction of the classical theory of time is a paradigm of strong formal ontological reconstructionism. A short account of this reconstruction and the motivation behind it may serve as an introduction to the subject of this chapter. The classical theory of time, as it occurred in classical mechanics, assumed that so-called instants of time constitute a one-dimensional continuum as characterized by Cantor. In the beginning of the twentieth century, logicians such as Russell and Whitehead gave a straightforward formalization of this theory. Such a formalization, from the point of view of weak formal ontological reconstructionism, reflects the ontological position that there are instants of time, ordered by an "earlier than" relation. This was indeed Whitehead's conclusion in 1906. From the point of view of strong formal ontological reconstructionism however, a defense of this ontological position is needed, as Russell realized in 1914. He argued that there is no other way of acknowledging such entities as instants of time than by postulation, since we cannot answer the epistemological question how we can know that such entities exist, given that they are "strictly instantaneous": impressions on our sense organs only produce sensations which are not merely and strictly instantaneous. Russell therefore sought to reconstrue the classical theory of time in such a way that it did not commit a philosopher to the existence of such entities as instants. The result of his investigations was an axiomatized theory reflecting the ontological position that there are events and temporal relations between events, whereas "what we can regard as an instant" was defined in terms of such events and their relations. According to Russell, this position could be defended with the argument that such entities as events are certainly known to exist. This was for Russell a reason for preferring his reconstruction to a reconstruction based on "inferred" (postulated) instants of time or whatever other kind of "disputable metaphysical entities". An ontological position was reached by means of a formal ontological reconstruction. The details will be given below.

This Part will be devoted mainly to the nature of formal ontological reconstructionism as found in the early writings of Whitehead and Russell. Some attention will be given to the possible roles which Hertz and Moore played in the development of this new kind of philosophy. Emphasis will be placed on the contrast with the logical analysis or, if one wishes, the rational reconstruction of mathematics given by Russell in The principles of mathematics. It will be argued that the latter kind of reconstructionism must not be interpreted as an attempt in "ontological reconstruction" - in this respect Russell sided with Frege. The particular position taken in The principles of mathematics, called "if-thenism", is not concerned with ontological questions, despite Russell's demand of a "philosophical discussion" of primitive notions.

Genuine ontological discussions can be found in Russell's treatment of the theories of space and time suggested by Leibniz and Newton (and Clarke). In order to evaluate these theories, Russell attempted to make them precise and this seems a first step towards a formalization of theories outside the field of pure mathematics. Though Frege foresaw this possibility already in his Begriffsschrift, Russell's approach was novel in that he regarded the formalizations as an attempt to make the ontological basis of the theories in question explicit, whereas Frege strived only for mathematical precision. For ontological questions however, we must look at the work of Hertz on mechanics; there will be a section on his ideas at the end of the first part of this chapter.

In The philosophy of Leibniz and his two articles in Mind (1901), Russell had not yet realized how a possible contribution to settling philosophical controversies could be made by a reconstruction of a particular theory of the material world in which one's ontological position is reflected in the choice of the fundamental entities and their relations. This happened only after he became acquainted with Whitehead's method of introducing defined entities as classes of fundamental entities.

I consider Whitehead such an important figure in the history of formal ontological reconstructionism that ample space will be given to his conscientious elaboration of leading ideas. There will be sections on his reconstructions, his formal apparatus, and his criteria.

Russell's doctrine was distinguished from Whitehead's by being called strong formal ontological reconstructionism. Russell also went further than Whitehead by his demand for sketches - not blueprints - of ontological reconstructions of so-called common-sense theories of the external world. This means that he tried to apply Whitehead's method to "some main problems of philosophy" which Moore had brought forth in his lectures in the winter of 1910-1911.

The background of strong formal ontological reconstructionism is, all in all, a complicated network of relations of ideas from different sides. Formal ontological reconstructions themselves can be rather complicated from a technical point of view.

CHAPTER FOUR

BEFORE FORMAL ONTOLOGICAL RECONSTRUCTIONISM

Logicism in The principles of mathematics

In order to see more clearly what was new in formal ontological reconstructionism, a short historical account of the earlier mentioned developments seems desirable. Notably Russell's logicism, seemingly so closely connected with ontological reconstructionism, has to be put in proper perspective. I shall argue that the logicist approach to mathematics, despite similarities with reconstructions of scientific theories, cannot be considered to involve ontological reconstructions, for logicist results did not have to satisfy ontological criteria.

In the second half of the nineteenth century, a new conception of mathematical rigour arose among mathematicians. A revision of the foundations of mathematical Analysis appeared to be necessary when it was seen that current proofs of theorems made an intuitive or tacit use of certain principles or postulates. Notably Weierstrass made important contributions to exactness in Calculus by revising earlier proofs. For example, he succeeded in giving an arithmetical proof of the first mean value theorem, where Cauchy's proof used properties of geometrical representations. It was Moritz Pasch who axiomatized mathematical theories. In 1882, he characterized the deductive process in geometry as follows (Pasch 1882a, p. 98):

> Es muss in der That, wenn anders die Geometrie wirklich deductiv sein soll, der Process des Folgerns überall unabhängig sein vom Sinn der geometrischen Begriffe, wie er unabhängig sein muss von den Figuren; nur die in den benutzten Sätzen, beziehungsweise Definitionen niedergelegten Beziehungen zwischen den geometrischen Begriffen dürfen in Betracht kommen.

This quotation from Pasch's Vorlesungen über neuere Geometrie has no wider scope than geometry. Moreover, Pasch failed to explain how to determine that a mathematical proof was indeed deductive. He did not mention the fact that Frege, as early as 1879, had presented a notational system which was meant to answer this question (1879a, p. IV):

> Sie soll also zunächst dazu dienen, die Bündigkeit einer Schlusskette auf die sicherste Weise zu prüfen und jede Voraussetzung, die sich unbemerkt einschleichen will, anzuzeigen, damit letztere auf ihren Ursprung untersucht werden könne.

Pasch's main concern was the presentation of a system of geometrical axioms (Grundsätze) such that all propositions (Lehrsätze) were logical consequences of them. He became famous for his discovery of implicit assumptions of traditional proofs in elementary geometry. But Frege also showed some interest in such questions. In his article "Ueber die wissenschaftliche Berechtigung einer Begriffsschrift", he pointed out how "such a conscientious and rigorous writer as Euclid" made tacit use of unspecified presuppositions (1882a, p. 50). The first two sections of Die Grundlagen der Arithmetik also make clear that Frege did not stand wholly outside "the movement, in favour of correctness in deduction, inaugurated by Weierstrass" (Russell 1903a, par. 107). He foresaw formalizations of geometry, mechanics and physics in the preface of Begriffsschrift (1879a, p. VI), but dealt only with logical analysis of arithmetic, which operates with logical concepts only. (According to Frege there is no sharp boundary between logic and arithmetic. From a scientific point of view, they form a single science - eine einheitliche Wissenschaft - (1885a, p. 95). Moreover, the universal applicability of arithmetic can only be explained by assuming that the arithmetical rules of inference are of a purely logical nature; the correctness of an arithmetical rule of inference cannot be founded in spatial "intuition" - räumliche Anschauung - for this would restrict at least a part of the arithmetical propositions to geometrical applications; <u>mutatis mutandis</u> the same holds for an alleged foundation in physical observations.)

On the other hand, Pasch and Hilbert tried to axiomatize Euclidean geometry without recourse to symbolic logic in whatever form. It seems that they shared the opinion of Paul du Bois-Reymond that for the evaluation of a proof, the logical conscience of professional mathematicians could be trusted (1882a, p. 11). Russell however, in the footsteps of Peano and other Italian mathematicians, recognized the importance of symbolic logic and of rigid formalism for the axiomatization of all sorts of mathematical theories. In 1901, he was convinced that mathematicians had the ability to treat the principles of mathematics in an exact manner, so he undertook this task and wrote <u>The principles of mathematics</u>.

In the second edition of this work, Russell said that at the time when he wrote <u>The principles</u>, he shared with Frege the belief in the Platonic reality of numbers, which, as he conceived it, peopled the timeless realm of Being ... Does this mean that the results which are reached in this book are of the kind aimed for by formal ontological reconstructionism?

I wish to defend the claim that Russell's analysis of mathematics in <u>The principles of mathematics</u> was not intended to throw any light on ontological matters. The argumentation will consist of three steps: First, I shall defend the view that Russell's if-thenism is just the sort of philosophy of mathematics that excludes questions of reality. Secondly, I shall show that Russell did not impose ontological criteria on the results of <u>The principles</u>; it will turn out that his analysis of the number concept did not have to wait for an answer to the ontological question of the reality of numbers. Third, I shall argue that Russell's discussion of the so-called logical constants is not ontological in character.

I start the first step by reviewing some general results. According to

Russell's analysis, "pure mathematics must contain no indefinables except logical constants, and consequently no premisses, or indefinable propositions, but such as are concerned exclusively with logical constants and with variables" (1903a, par. 9). In other words: "pure mathematics is the class of all propositions of the form "p implies q", where p and q are propositions containing one or more variables, the same in the two propositions, and neither p nor q contains any constants except logical constants" (par. 1) [23]. It is clear why this position is called "if-thenism"; it says that mathematical statements have the form: <u>if</u> a certain assertion is true of any set of entities, <u>then</u> some other assertion is true of those entities. It leaves the question open whether a model for a mathematical theory actually exists. Hence if-thenism does not engage one in ontological problems.

There is a difficulty: given that the if-then-statements require the so-called logical constants for their formulation, must they themselves not be considered "entities" in an ontological sense? For an answer to this question, I distinguish between philosophical and non-philosophical discussions in <u>The principles</u>.

As regards the non-philosophical discussions of <u>The principles of mathematics</u>, Russell's position on logical constants can be read off from his preference for calling them "notions". The reason why they are also called "constants' is that "every such logical or mathematical notion as a constant is to be something absolutely definite, concerning which there is no ambiguity whatever" (par. 7). They are obtained primarily as "the necessary residue in a process of analysis" (<u>Preface</u>). It appears that logical constants are all notions definable in terms of: <u>implication</u>, the relation of a term to a class of which it is a member, the notion of <u>such that</u>, the notion of <u>relation</u>, and some further notions, such as <u>propositional function</u>, <u>class</u>, <u>denoting</u>, and <u>any</u> or <u>every term</u> (cf. Russell 1903a, par. 1 and par. 106).

I am aware that there are problematic uses of the word 'notion', for example in Berkeley's philosophy. Since in <u>The principles of</u>

mathematics no place can be pointed out in which Russell used the term 'notion' ontologically, I assume that he followed common mathematical practice [24].

Philosophical discussions concerning "entities" can be found above all in Chapter LI of The principles, which was almost completely taken over from Russell's paper "Is position in Time and Space absolute or relative?". Here Russell introduced a distinction between "being" and "existence" (Russell 1901c, p. 310): "Being is that which belongs to every conceivable term, to every possible object of thought." "Existence, on the contrary, is the prerogative of some only amongst beings." Something "is", or has being, as soon as it can be counted. The approximately twelve primitive notions of logic and mathematics have being, and so do all logical constants definable in terms of them. But they do not exist in the sense in which 'existence' is used in philosophy and in daily life (Russell 1905a, p. 398):

> The meaning of existence which occurs in philosophy and in daily life is the meaning which can be predicated of an individual: the meaning in which we inquire whether God exists, in which we affirm that Socrates existed, and deny that Hamlet existed. The entities dealt with in mathematics do not exist in this sense: the number 2, or the principle of the syllogism, or multiplication are objects which mathematics considers, but which certainly form no part of the world of existent things. This sense of existence lies wholly outside Symbolic Logic, which does not care a pin whether its entities exist in this sense or not.

Since the notion of "existence" occurring in mathematics or symbolic logic does not discriminate in the class of "objects" with which these disciplines deal, ontological questions seem of no relevance here. That "numbers, the Homeric gods, relations, chimeras and four-dimensional spaces all have being, for if they were not entities of a kind, we could make no propositions about them" (Russell 1901c, p. 310), does not make them entities in an ontological sense. Therefore my claim that if-thenism has no concern with ontological questions withstands Russell's philosophical discussion concerning entities.

The outstanding result of the first two parts of The principles lies in Russell's analysis of the number concept. I shall now undertake the second step of my argument and examine if this result had to satisfy ontological criteria. Consider Russell's conclusion and justification of his analysis (par. 111):

> Mathematically, a number is nothing but a class of similar classes: this definition allows the deduction of all the usual properties of numbers, whether finite or infinite, and is the only one (as far as I know) which is possible in terms of the fundamental concepts of general logic.

In my view, this has nothing to do with a supposed ontological status of numbers. On the contrary, the above account shows how little has been done when analytical problems have been solved: the ontological problem what numbers "are" or whether they "exist" is still left open. Russell did not fail to acknowledge this (par. 111):

> But philosophically we may admit that every collection of similar classes has some common predicate applicable to no entities except the classes in question, and if we can find, by inspection, that there is a certain class of such common predicates, of which one applies to each collection of similar classes, then we may, if we see fit, call this particular class of predicates the class of numbers.

But he then went on to state explicitly that his final acceptance of the given logical analysis is not influenced by any answer to the ontological problem:

> For my part, I do not know whether there is any such class of

> predicates, and I do know that, if there be such a class, it is wholly irrelevant to Mathematics. Wherever Mathematics derives a common property from a reflexive, symmetrical, and transitive relation, all mathematical purposes of the supposed common property are completely served when it is replaced by the class of terms having the given relation to a given term; and this is precisely the case presented by cardinal numbers. For the future, therefore, I shall adhere to the above definition, since it is at once precise and adequate to all mathematical uses.

In other words, a philosopher might not be content with the technical invention of equivalence classes, but may want to discover common properties which are the ontological ground of the "fact" that the relation used is indeed an equivalence relation. One who talks in this way can be considered a Platonist who believes in the reality of arithmetical relations. Russell surely was such a Platonist in The problems of philosophy; he might have been a Platonist at the time he wrote The principles. But the above quotation indicates that he stuck to "mathematical" criteria of adequacy in the realization of his program of logical analysis of mathematics.

That is not to say that there is no relation at all between Russell's philosophical beliefs and his analytical treatments. In the following fragment, we see that Russell distinguished between the origin of a certain treatment of relations and its adequacy for a logical analysis of mathematics. Concerning the way in which Peirce and Schroeder built a theory of relations, Russell wrote (Russell 1903a, par. 27):

> In addition to the defects of the old Symbolic Logic, their method suffers technically (whether philosophically or not I do not at present discuss) from the fact that they regard a relation essentially as a class of couples, thus requiring elaborate formulae of summation for dealing with single relations. This view is derived, I think, probably

> unconsciously, from a philosophical error: it has always been customary to suppose relational propositions less ultimate than class-propositions (or subject-predicate propositions, with which class-propositions are habitually confounded), and this has led to a desire to treat relations as a kind of class. However this may be, it was certainly from the opposite philosophical belief, which I derived from my friend Mr. G. E. Moore, that I was led to a different formal treatment of relations. This treatment, whether more philosophically correct or not, is certainly far more convenient and far more powerful as an engine of discovery in actual mathematics.

Accordingly we can distinguish between three stages: first there is a pre-analytic stage of philosophical beliefs. These may influence the second stage, that of formal analytical treatments. Thirdly, there is a post-analytic stage of "philosophical logic", as Russell called it, which consists of a philosophical discussion of the findings of the second stage. The above example of Russell's analysis of the number concept shows that the conclusions reached in the second stage cannot be justified by an appeal to the first stage; the discussion has to wait for the third stage. This shows that analytical treatments in the sense of _The principles of mathematics_ cannot be considered to have the aim of establishing an ontological position. Nevertheless I admit that Russell came very close to the view that one's philosophical position is reflected in some way in the basic assumptions of one's analytical treatment. However, there is a distinction between the fact that an assumption has been _derived_ from a philosophical doctrine, and the opinion that an assumption _expresses_ a philosophical doctrine. Russell remained cautious about this distinction when he wrote (Russell 1903a, Preface):

> The doctrines just mentioned are, in my opinion, quite indispensable to any even tolerably satisfactory philosophy of mathematics, as I hope the following pages will show. But

> I must leave it to my readers to judge how far the reasoning assumes these doctrines, and how far it supports them. Formally, my premisses are simply assumed; but the fact that they allow mathematics to be true, which most current philosphies do not, is surely a powerful argument in their favour.

Russell paid considerable attention to what he called philosophical logic. He wanted an explanation of the fundamental concepts which mathematics accepts as indefinable (Preface). This brings me to the third step, the outcome of which is that this discussion too is not ontological. Now Russell's account of the matter can be misleading if no attention is payed to how the discussion actually proceeds and the following quotation is taken in isolation:

> The discussion of indefinables - which forms the chief part of philosophical logic - is the endeavour to see clearly, and to make others see clearly, the entities concerned, in order that the mind may have that kind of acquaintance with them which it has with redness or the taste of a pineapple. Where, as in the present case, the indefinables are obtained primarily as the necessary residue in a process of analysis, it is often easier to know that there must be such entities than actually to perceive them; there is a process analogous to that which resulted in the discovery of Neptune, with the difference that the final stage - the search with a mental telescope for the entity which has been inferred - is often the most difficult part of the undertaking. In the case of classes, I must confess I have failed to perceive any concept fulfilling the conditions requisite for the notion of class. And the contradiction discussed in Chapter x. proves that something is amiss, but what this is I hitherto failed to discover.

It looks as if Russell considered the task of elucidating the primitive logical distinctions a fundamental one, whereas Frege attributed it only to a propaedeutic level outside the system of science (1906a, p. 301). According to Frege, the aim of elucidations is a practical one, to wit, the mutual understanding of the investigators. One has to be satisfied with the elucidations as soon as this practical aim has been reached. Russell, on the other hand, attributed to philosophy not only the discovery of the principles of deduction, but also the recognition of indefinable entities and the ability to distinguish between such entities. Philosophical questions had to be settled by "inspection" rather than by accurate chains of reasoning. However, it is doubtful whether this meant much in practice. Russell himself remarked that philosophical argument, strictly speaking, consists mainly of an endeavour to cause the reader to perceive what has been perceived by the author. "The argument, in short, is not of the nature of proof, but of exhortation." (1903a, par. 124).

Russell's discussion of the "essentially philosophical question" whether there is any indefinable set of entities commonly called numbers, different from the set of entities defined above (par. 124), indeed amounted to a reconsideration of the notions of _one_ (par. 125), _class_ (par. 126, 127), _a term_ (par. 128), counting (par. 129), numerical conjunction and collection (par. 130), addition (par. 131) and _any term_ (par. 132); but this reconsideration was mainly restricted to an informal discussion why his procedure of defining the number 1 did not presuppose the notion of 1. Nowhere did Russell appeal to "perception" in a sense of "immediate apprehension" (par. 124); instead, he more than once adduced a technical argument from the development of the theory, e.g. "to identify the two relations which Peano distinguishes (_i.e._ the relation of an individual to its class, and the relation of a class to another in which it is contained; H.V.) "is to cause havoc in the theory of infinity, and to destroy the formal precision of many arguments and definitions" (par. 125).

My evaluation is the same as in the foregoing case: Russell's discussion does not give support to the view that the results of his analysis have to satisfy ontological criteria. His philosophical logical discussion was nothing more than a critical review of these results.

Given this conclusion, the natural question is: what was Russell looking for, when he wanted a direct insight into a notion? Was it a kind of "psychological simplicity" or can his struggle with the notion of class be attributed to his inability to understand Frege's theory of value ranges, which had to be incorporated into his own theory at a stage in which his book on the principles of mathematics was almost finished? We know that Russell asked Frege to explain some points of the theory of value ranges, and was soon convinced that these value ranges could not be identified with numerical conjunctions, though they had to be classes which are determined when their members are given. In 1902, Russell could still make some additions into the main text of his book on the basis of his newly acquired conviction, but he hinted here and there that he was not completely satisfied with this Fregean notion of class [25]. I already quoted Russell's confession that he failed to perceive any concept fulfilling the conditions requisite for the notion of class. There are more statements of this tenor, notably Russell's reaction to Frege's explanation in his letter to Frege dated 8.8.1902 (Frege 1976a, p. 226):

> Besten Dank für Ihre Erklärungen über Werthverläufe. Ich verstehe jetzt die Nothwendigkeit die Werthverläufe nicht bloss als Aggregat von Gegenständen, als System, zu behandelen. Noch immer aber fehlt mir gänzlich die directe Anschauung, die directe Einsicht, dessen was sie Werthverlauf nennen: logisch ist er nothwendig, aber er bleibt für mich eine gerechtfertigte Hypothese.

Here, Russell again placed himself into the position of the "philosophical logician" who asks for "immediate inspection". However, as far as his standpoint of 1903 is concerned, this position did not bring him to question his logical analysis of numerical statements.

This can be seen in the following passage (Russell 1903a, par. 132):

> The assertion of numbers depends upon the fact that a class of many terms can be a logical subject without being arithmetically one. Thus it appeared that no philosophical argument could overthrow the mathematical theory of cardinal numbers set forth in Chapters xi to xiv.

The idea of a formalization of non-mathematical theories

Formal ontological reconstructionism presupposes the possibility of formalization. Frege himself did realize that a formalization of theories outside pure mathematics might be given with the help of his logical theory: in the foreword to Begriffsschrift he declared his formula language applicable not only to arithmetic and the foundation of the differential and integral calculus, but also to geometry, pure kinematics, mechanics and physics. However, he did not make clear how this could be done, except for geometry, where "only a few symbols for intuitive relations had to be added" (Frege 1879a, p. VI):

> Es müssten nur für die hier vorkommenden anschaulichen Verhältnisse noch einige Zeichen hinzugefügt werden.

That a formalization of geometry had to use such symbols is in accordance with Frege's standpoint that geometrical propositions are synthetic, and even synthetic a priori. On this question Frege agreed with Kant, as can be gathered from Die Grundlagen der Arithmetik (Frege 1884a, par. 89, cf. par. 103). This does not mean that Frege can be regarded as a forerunner of formal ontological reconstructionism. For him, the purpose of formalization was solely to gain greater precision and to discover inaccuracies in proofs - notably in geometry. Such formalization can be seen as the result of a logical analysis without any particular relevance to ontological questions.

Nevertheless, there is one place in Die Grundlagen der Arithmetik in which Frege comes very close to the idea of laying restrictions on formalizations of geometry: one of his arguments against a definition of the expression "parallel" in terms of "direction" was that everything geometrical must be primarily "intuitive" - anschaulich - (Frege 1884a, p. 75):

> Nun frage ich, ob jemand eine Anschauung von der Richtung einer Gerade hat. Von der Gerade wohl! aber unterscheidet man in der Anschauung von dieser Gerade doch noch ihre Richtung? Schwerlich! Dieser Begriff wird erst durch eine an die Anschauung anknüpfende geistige Thätigkeit gefunden. Dagegen hat man eine Vorstellung von parallelen Geraden.

Precisely this idea points into the direction of the fundamental principle of "philosophical reconstructions" of a body of knowledge: a philosophical reconstruction purports to reflect one's philosophical position regarding the body of knowledge in question. Defining the concept "parallel" in terms of the concept "direction" was for Frege turning the matter the other way round. ("Nur schade, dass der wahre Sachverhalt damit auf den Kopf gestellt wird!") Apparently Frege considered the concept "parallel" "epistemologically prior" to the concept "direction", so this priority had to be shown by the order of reconstruction. It is worth noticing, however, that the idea of an ontological reconstruction is absent in the above quotation. This is in accordance with my claim that such an idea is nowhere present in Frege's approach.

It seems that the idea of an ontological reconstruction was already part of Russell's early attempts to make non-mathematical theories precise in such a way that the process of formalization was facilitated. This brings me to a consideration of Russell's approach to theories of time in his article "Is position in time and space absolute or relative?" (Russell 1901c). The discussion will be preceded by a short background sketch, and closes with a brief treatment of the first steps towards a reconstruction of a theory of the material world in the last part of The principles of mathematics.

In his book on the philosophy of Leibniz, Russell discussed the different theories of space and time which played such an important role in the discussion between Leibniz and Clarke. According to Russell, their controversy can be summarized as follows (Russell 1900a, par. 61):

> If we take two points A and B, they have (1) a distance, which is simply a relation between the two, (2) an actual length, consisting of so much space, and stretching from A to B. If we insist on the former as the essence of space, we get a relational theory; the terms A and B, whose distance is spatial, must themselves be non-spatial, since they are not relations. If we insist on the latter, the actual intervening length, we find it divisible into an infinite number of points each like the end points A and B. This alternative gives the Newtonian theory of absolute space, consisting, not in an assemblage of possible relations, but in an infinite collection of actual points.

But that was not the end of the matter. After this lucid statement of the two theories, Russell criticized both Newton's and Leibniz's theory, respectively for being "self-contradictory", and "inconsistent with the facts and in the end, just as self-contradictory". He pointed out that "a theory free from both these defects is much to be desired, as it will be something which philosophy has not hitherto known". As we shall see, Russell himself later tried to establish such a theory of time and space. But before doing so, he had to reformulate the different theories in a more precise way. Indeed a first attempt was already made in Russell's study of Leibniz. A full quotation of Russell's formulation of a relational theory of time in A critical exposition of the philosophy of Leibniz seems desirable. It enables us to see what was new in later ontological reconstructions: in the earlier versions, instants of time are not among the fundamental entities, nor do they reappear as "defined entities"; in the later version, instants of time are not part of the fundamental entities either, but they are "reconstructed" in terms of the fundamental entities. As we shall see, the latter approach at least has the advantage that we can keep talking of "instants of time" without thereby implying that there are such things as the pre-systematically assumed instants of time. Certain paradoxical consequences of the earlier forms of the relational theory are thus eliminated.

After having remarked that Leibniz did not seem to have perceived clearly what is involved in the relativity of time, Russell made the following refinements (Russell 1900a, par. 72):

> What is involved is, that in time, as in space, we have only distances, not lengths or points. That is, we have only <u>before</u> and <u>after</u>: events are not at a certain time, but those which are not simultaneous have a distance, expressed by saying that one is before the other. This distance does not consist of points of time, so that we cannot say time has elapsed between two events. Other events may be between them - <u>i.e.</u> there may be events before one of our pair and after the other. But when two events have no event between them, they have merely a relation of before and after, without being separated by a series of moments. No event can last for any length of time, for there is no such thing as a length of time - there are only different events forming a series. Nor can we say that events last for an instant, since there are no instants.

How this theory deviates from ordinary usage can be seen from the resulting theory of motion. For example, it can not be expressed in the language of the theory that a body is either in rest or in motion, in the usual understanding of the words. But there are alternatives:

> To say that a body is at rest, can only mean that its

> occupancy of a certain position in space is simultaneous (simultaneity being an ultimate relation) with two events which are not simultaneous with each other. And to say that a body is in motion will mean that its occupancy of one position and its occupancy of another are successive. But from this we shall never arrive at a state of a motion, even by taking an infinite number of spatial positions successively occupied.

In the end, a relational theory of time seemed quite possible. However, Russell reopened the discussion in his article "On the notion of order". He here said that "grave difficulties would arise if we were to regard the time-series as primarily one of events" (Russell 1901a, p. 47), "But when it is recognised that events only acquire an order by correlation with the times they occupy, no difficulties emerge" (Russell 1901a, p. 47-48). This choice for an absolute theory of time was defended in Russell's next Mind-article, "Is position in time and space absolute or relative?". The argumentation was directed against Lotze, who had argued that absolute theories are logically impossible. The question was that Lotze - from a modern point of view - had no adequate theory of relations. According to Russell, "the theory of relations pronounced by Lotze is, in fact, a theory that there are no relations" (Russell 1901b, p. 308). That is to say, Lotze had a logical theory which analysed every proposition as having the subject-predicate form in such a way that there is always only one subject. Russell did not hesitate to reduce this theory to the absurd (Russell 1901b, p. 309-310). More positively, he showed that it is possible to state both the absolute and the relative theories of time and space with the help of the language of his theory of relations set forth in the preceding Mind-article "On the notion of order". The results contain several elements of the later reconstructionism, but also exhibit features which do not come back in for instance Russell's 1914 work on time.

Russell's account of the absolute theory of time can be summarized as follows:

(a) we have two classes of entities, (1) those which are positions in time, or "moments", (2) those which have positions, or "qualities";

(b) any two moments have an asymmetrical transitive relation, either before or after;

(c) each quality has a certain specific, transitive asymmetrical relation to one or more of the moments, namely the relation of being at, or "occupying", the moment;

(d) the compound - *i.e.* ordered pair - of a given quality at a given time is called an event; two events are called simultaneous when both are "qualities at one time", in other words when both are in one and the same moment; otherwise they are called successive.

It is easy to see that a formalization of these statements offers no problems, certainly not for the author of "Sur la logique des relations avec des applications à la théorie des séries" (Russell 1902a). Nor do proofs of some consequences of the above axioms and definitions, such as the proposition that the relation in (of an event to "its" moment) is intransitive and asymmetrical. A special consequence is that a given event can be only in one moment. This proposition can be proved logically from definitions alone. Russell expressed this by saying that an event is logically incapable of recurrence.

We see that this absolute theory of time assumes that there are two kinds of fundamental entities, qualities and moments. It also assumes two kinds of fundamental time relations, relations holding between moments, and those between qualities and moments. Events are defined entities; they do not form a kind of fundamental entity. But in the absolute theory, we can in a sense talk about "events", as we do presystematically. For example, we can formulate in the new terminology that one (defined) event is (formally) simultaneous with, or (formally) successive to another (defined) event. Another example is the above-mentioned statement that a (defined) event is incapable of (formal) recurrence. Pre-systematically, it is indeed the case that an event is incapable of recurrence as soon as one realizes that "Whatever can, in ordinary language, recur or persist, is not an event" (Russell 1901b, p. 295). But is this a logical truth? There are indications that Russell posed this problem in the context of a discussion of the relational theory. It thus seems desirable to consider Russell's account of this theory, which can be summarized as follows:

(a) we have a single class of entities, called events;
(b) any two events have one and only one of three (unanalyzable) relations, simultaneity, priority and posteriority. These relations are all transitive; the first is symmetrical, the other two asymmetrical. Moreover, if A is simultaneous with B and B is before C, then A is before C; if B is after C, then A is after C.

Strangely enough, the above account of a relational theory is not a reconstruction of an available pre-systematic relational theory of time. Russell simply assumed that such a theory recognizes homogeneous relations only, and that the postulated members of the union of their fields are called events. This appears from the fact that he asked afterwards whether the sketched theory applies to the real world. And indeed, his answer is negative (Russell 1901b, p. 295):

> The relational theory may seem, at first sight, simpler than the absolute theory, but in its application a great difficulty arises from the absence of any such class of entities as the events which it requires.

This does not mean - pace Lotze - that the relational theory of time is self-contradictory, only that "in the case of time, provided we take any account of the facts, it is impossible to free the relational theory from contradiction" (Russell 1901b, p. 293). In other words, the relational theory is inconsistent with the facts. At one point Russell concluded that events in the sense of this theory either cannot be found, or must be identified with the kind of entities which the absolute theory assumes. A more convincing argument that Russell's version of the relational theory did not result from a deliberate choice of the fundamental entities can scarcely be given. This circumstance may have influenced his attitude towards the status of the axioms of the calculus which had still to be interpreted. Notably the axiom that the relation of priority is asymmetrical was considered to express that it is logically impossible for the members of its field to recur or persist. No wonder then, that Russell could not find suitable candidates for the "events" in the sense of the theory. For even the most plausible non-recurring events, namely those that are complex, do not satisfy this condition (Russell 1901b, p. 295):

> The death of Caesar or the birth of Christ, it may be said, were unique: they happened once, but will never happen again. Now it is no doubt probable that nothing exactly similar to these events will recur; but, unless the date is included in the event, it is impossible to maintain that there would be a logical contradiction in the occurrence, in the future, of a precisely similar event.

Furthermore, it does not help when we resort to "states of the universe" such that any two of them have an asymmetrical transitive relation, either before or after, with events as parts of such states so that before and after do not hold between them directly, but only by correlation. Such a theory, Russell argued, "is merely the absolute theory with states of the whole universe indentified with moments" ("except for the fact that at is no longer simple"). As we shall see, this is almost Russell's later theory of time, in which states of the universe (in the sense of maximal groups of mutually simultaneous events) do play the role of moments. In this theory, the fundamental entities are ordinary events, incapable of recurrence, for which the relation of simultaneity is only transitive, simply because, pre-systematically speaking, they "last or persist for some time". In 1901, Russell could not yet "construct" moments on the base of a relational theory of time; on the contrary, as we can see from the following discussion (Russell 1901b, p. 295):

> If two events were simultaneous because they had some common property not shared by events not simultaneous with them, the collection of all such common properties of different groups of simultaneous events would have exactly the characteristics by which we defined absolute time. Now for my part I consider it self-evident that all symmetrical transitive relations are analysable; and if this axiom were admitted, the relational theory would fall at once. But as my opponents will allow no such principle, I shall not appeal to it in what follows.

We have seen that Russell soon afterwards, in The principles of mathematics, replaced the just-mentioned principle by the principle of definition by abstraction. This enabled him to define cardinal numbers logically without an appeal to common properties. However, the insight that the principle could also be used for ontological reconstructions came later. Yet a new idea did appear in the last part of The principles of mathematics which would become very fruitful for the future development of formal ontological reconstructionism. It arose within the context of Russell's interest in characterizations of the purely mathematical or logical structure of theories.

In section 415 of The principles, Russell tried to show "that the definition of a kind of space is always possible in purely logical terms, and that new indefinables are not required". He did this by defining a space as any class of entities such that it always has a certain number of members for which certain axioms hold. In this way, geometry became part of pure mathematics. The question as to whether a certain theory of space applies to the actual world was considered irrelevant to pure mathematics (Russell 1903a, par. 423). In other words, there is "no logical implication of other entities in space" (Russell 1903a, par. 438):

> It does not follow, merely because there is space, that therefore there are things in it. If we are to believe this,

> we must believe it on new grounds, or rather on what is called the evidence of the senses. Thus we are here taking an entirely new step.

The new step Russell was talking of is the recognition of fundamental entities which are considered to exist in the actual world (o.c.):

> Among terms which appear to exist, there are, we may say, four great classes: (1) instants, (2) points, (3) terms which occupy instants but not points, (4) terms which occupy both points and instants. What is meant by occupying a point or an instant, analysis cannot explain; this is a fundamental relation, expressed by in or at, asymmetrical and intransitive, indefinable and simple.

Here we have at least an indication how a formalization of a theory of the material world might start. Russell went even further when he tried to answer the question how to distinguish bits of matter from whatever else exists in space, such as the so-called secondary qualities. He found a solution in the nature of the connection of matter with space and time (Russell 1903a, par. 440): "Two pieces of matter cannot occupy the same place at the same moment, though it may occupy two moments at the same place". Eventually it enabled Russell to give "an abstract logical statement" of the subject matter of rational Dynamics, considered as a branch of pure mathematics: he replaced time and space by a one-dimensional and n-dimensional series respectively, and material points by relations between those series (Russell 1903a, par. 144). This, of course, no longer has anything to do with an ontological reconstruction of a theory of the material world [26]. But Russell at least showed that it is possible to conceive the material world as "a set of entities which occur as forming the 'fields' of these relations". This is the formulation of Whitehead in his memoir "On mathematical concepts of the material world". How he came to see the significance of Russell's new step, whereas Russell limited himself to characterizations of mathematical structures, is another story. This will be dealt with in the following chapter.

First ideas about ontological reconstructionism

When Whitehead wrote his important memoir "On mathematical concepts of the material world", he made use of the last part of The principles of mathematics, in which Russell discussed the subject "matter and motion". Russell, in his turn, showed acquaintance with Hertz's and Kirchhoff's treatises on mechanics. (Cf. Russell 1903a, par. 448 and 470-472.) His contention that "the material universe" consisted of instants of time, points of space, and pieces of matter or particles can immediately be traced back to views of Kirchhoff and Hertz. The following brief account of some of these views aims to make clear that Hertz was, in a sense, a direct precursor of Whitehead and Russell in matters of ontological reconstructionism. It is not accidental that he gave his treatise on mechanics the title 'Die Prinzipien der Mechanik in neuem Zusammenhange dargestellt'.

Hertz tried to show that Kirchhoff was right in pointing out that the ideas - Vorstellungen - of space, time and matter are "necessary and sufficient for the development of mechanics" (cf. Hertz 1894a, p. 29-30). A concept of force or energy could be "abolished as independent basic idea" - als selbständige Grundvorstellung beseitigt - that is to say, Hertz assimilated the concept of force into his system as "an auxiliary mathematical construction" - eine mathematische Hilfskonstruktion - . This was in the spirit of Kirchhoff, who wrote that the science of mechanics had to construe the required auxiliary concepts, such as the concepts of force and mass (cf. Kirchhoff 1874b, p. 1). Kirchhoff's main reason for his approach was the greater simplicity which it brought; his position was instrumentalistic in the sense that in physics a mathematical theory presents only a calculus which still has to be observationally interpreted; the mathematical equations cannot be regarded as a description of the physical world, but they can make (simple) descriptions possible. This was pointed out in the Prospectus for the serial publication of Kirchhoff's Vorlesungen über analytische Mechanik (Kirchhoff 1874a, p. 2):

> Der Verfasser bezeichnet es nämlich als die Aufgabe der Mechanik, die in der Natur vor sich gehenden Bewegungen vollkommen und auf die einfachste Weise zu b e s c h r e i b e n, und begründet, hiervon ausgehend, unter Voraussetzungen der Vorstellungen von Raum, Zeit und Materie, die Lagrange'schen Gleichungen durch rein mathematische Betrachtungen. Freilich erscheinen diese Gleichungen dann als solche, die über die wirklichen Bewegungen der Körper gar nichts aussagen; sie bilden nur ein Schema für diese, dem Inhalt zu geben Sache der Beobachtung ist; ihr Nutzen beruht darauf, dass sie eine Sprache möglich machen, die, wie die Erfahrung gelehrt hat, sich besonders eignet, die wirklichen Bewegungen in einfacher Weise zu beschreiben.

Another reason why Kirchhoff did not accept the usual treatments of mechanics as a science of forces in the sense of "causes which produce or strive to produce movements" is given in the foreword to the "official" edition of the Vorlesungen über analytische Mechanik: the unclarity of the ordinary way of dealing with "forces", "causes" and "strivings" leads to obscurities which he wanted to eliminate. He therefore made the proposal to restrict the task of the science of mechanics in the indicated way. It is true that the concept of force reappeared, but with a different function (Kirchhoff 1876a, p. III-IV):

> Man hat auch auf diesem Wege es mit dem Begriffe der Kraft zu thun und ist nicht im Stande, eine vollständige Definition desselben zu geben. Die Unvollständigkeit dieser Definition hat hier aber keine Unklarheit zur Folge, da die Einführung der Kräfte hier nur ein Mittel bildet, um die Ausdruckweise zu vereinfachen, um nämlich in kurzen Worten Gleichungen auszudrücken, die ohne Hülfe dieses Names nur schwerfällig durch Worte sich würden wiedergeben lassen. Hier reicht es aus, um jede Dunkelheit zu entfernen, die Kräfte soweit zu definiren, dass jeder Satz der Mechanik, in dem von Kräften die Rede ist, in Gleichungen übersetzt werden kann; und das

geschieht auf dem eingeschlagenen Wege.

The result was a reconstruction of Newtonian mechanics. But I would not call it a philosophical reconstruction, let alone an ontological reconstruction, since Kirchhoff gave only methodological arguments in its favour.

Hertz saw the matter differently and did not present only methodological considerations. It is well known that he distinguished three criteria for comparisons between different "pictures" as he called it - Bilder - of the external world: (logical) admissibility - Zulässigkeit - , correctness - Richtigkeit - , and appropriateness - Zweckmässigkeit - . The last criterion included aspects such as significance, simplicity and economy or parsimony, which are not philosophical criteria by themselves. However, especially on parsimony, Hertz's considerations went considerably further than Kirchhoff's remarks. This can be seen from Hertz's application of the above criteria to the traditional theory of mechanics based on the concepts of time, space, force and mass. Writing about the question of the many forces which are assumed in this theory, Hertz granted that "inessential side-connections" - unwesentliche Nebenbeziehungen - cannot be completely avoided, though every principle of parsimony would demand the utmost possible curtailment of such connections. It can be asked to what extent classical mechanics obeys this (Hertz 1894a, p. 15):

> Kann man aber behaupten, dass die Physik in dieser Richtung immer mit Sparsamkeit zu Wege gehen konnte? Musste sie nicht vielmehr die Welt bis zum Übermass erfüllen mit den verschiedensten Arten von Kräften, mit Kräften, welche selbst niemals in die Erscheinung treten, sogar mit solchen, welche nur ganz ausnahmsweise überhaupt eine Wirkung haben?

Hertz sketched how this applies to the physical theory built upon traditional mechanics with the help of the example of a piece of iron resting on the table in which a tremendous amount of forces would be in equilibrium. He then asked (1894a, p. 15-16):

> Wenn wir nun diese Vorstellungen unbefangen Denkenden vortragen, wer wird uns glauben? Wen werden wir überzeugen, dass wir noch von wirklichen Dingen reden und nicht von Gebilden einer ausschweifenden Einbildungskraft? Wir selbst aber werden nachdenklich werden, ob wir wirklich die Ruhe des Eisens und seiner Teile in einfacher Weise geschildert und abgebildet haben. Ob sich die Verwickelung überhaupt vermeiden lässt, ist zunächst ja fraglich; aber das ist nicht fraglich, dass ein System der Mechanik, welches sie vermeidet oder ausschliesst, einfacher und in diesem Sinne zweckmässiger ist, als das hier betrachtete, welches solche Vorstellungen nicht nur zulässt, sondern uns geradezu aufzwingt.

Similar objections were made against a second system of mechanics, which assumed four basic concepts. From the way in which Hertz wrote about these concepts, I presume that he considered this theory from an ontological point of view (Hertz 1894a, p. 18):

> Zwei derselben haben einen mathematischen Charakter: Raum und Zeit; die beiden anderen: Masse und Energie, werden eingeführt als in gegebener Menge vorhandene, unzerstörbare und unvermehrbare physikalische Wesenheiten.

This is confirmed by the following considerations, in which Hertz's own theory comes up in a way which would not be unbecoming for a philosopher (Hertz 1894a, p. 30-31):

> Wollen wir ein abgerundetes, in sich geschlossenes, gesetzmässiges Weltbild erhalten, so müssen wir hinter den Dingen, welche wir sehen, noch andere, unsichtbare Dinge vermuten, hinter den Schranken unserer Sinne noch heimliche

> Mitspieler suchen. Diese tieferliegenden Einflüsse erkannten wir in den ersten beiden Darstellungen an und wir dachten sie uns als Wesen einer eigenen und besonderen Art, deshalb schufen wir zu ihrer Wiedergabe in unserem Bilde die Begriffe der Kraft und der Energie. Es steht uns aber noch ein anderer Weg offen. Wir können zugeben, dass ein verborgenes Etwas mitwirke und doch leugnen, dass dieses Etwas einer besonderen Kategorie angehöre. Es steht uns frei anzunehmen, dass auch das Verborgene nichts anderes sei als wiederum Bewegung und Masse, und zwar solche Bewegung und Masse, welche sich von der sichtbaren nicht an sich unterscheidet, sondern nur in Beziehung auf uns und auf unsere gewöhnlichen Mittel der Wahrnehmung. Diese Auffassungsweise ist nun eben unsere Hypothese.

In other words, Hertz assumed that there exist visible as well as invisible masses, both belonging to one and the same - I venture to say - ontological category, and obeying the same laws. Accordingly, Hertz endeavoured to show that in this arrangement, the content of his science did not turn out less rich and general than the content of a mechanics starting from four basic ideas, at least as far as "the representation of nature" required (Hertz 1894a, p. 33). It is true that the concept of force was reintroduced, but no ontological commitment was involved (Hertz 1894a, p. 33):

> Uberigens erweist es sich auch hier bald als zweckmässig, den Begriff der Kraft einzuführen. Aber die Kraft tritt nun nicht auf als etwas von uns unabhängiges und uns fremdes, sondern als eine mathematische Hilfskonstruktion, deren Eigenschaften wir völlig in unserer Gewalt haben, und welche also auch für uns nichts Rätselhaftes an sich haben kann.

As a result, Hertz's mechanics can be seen as a reconstruction of classical mechanics in which the basic assumptions of the treatment explicitly reflect an ontology: times, spaces and masses are "symbols for objects of external experience" - Zeichen für Gegenstände der äusseren Erfahrung - (Hertz 1894a, p. 157), forces are not. But it was not a formal reconstruction in the sense that the precise methods of deduction and definition of mathematical logic were used. Hertz did not even need informal axiomatizations in the purely mathematical part of his theory; here he declared "time" simply to be "the time of our inner intuition", and "space" to be "the space of our presentations". He did not deal with the question of the structure of this kind of time other than in very general terms, and assumed that this kind of space was the same as the space of Euclidean geometry. He also assumed that the application of geometry to physically measured spatial relations did not lead to contradictions. His only argument was an appeal to "experience", not realizing that there is a problem of making Euclidean geometry at least formally applicable. A formal reconstruction of a theory of the material world would have to solve this kind of problem.

Is it possible to give a rational reconstruction of Hertz's theory? Let us see what would be needed for this. First of all, Hertz's system comprises an independent, or self-contained theory of time and a similar theory of space. Hertz's theory of time assumes one kind of fundamental object, "times" - Zeiten - , apparently with a fundamental relation ("after") such that the set of all times is continuously ordered by this relation. (Cf. Hertz 1894a, section 298.) The problem of characterizing a one-dimensional continuum was solved shortly afterwards by Cantor in Part I of his "Beiträge zur Begründung der transfiniten Mengenlehre" (Cantor 1895a). Formalizations were available in the beginning of the twentieth century; Russell gave an informal presentation in The principles of mathematics, based on a formalization in his article "Sur la logique des relations". Hertz's theory of space was Euclidean metrical geometry. Its fundamental objects were "points of space" - Punkte des Raumes - . (For the rest Hertz did not have much to say about this theory; cf. Hertz 1894a, section 299.) The problem of characterizing the structure of Euclidean space was treated after Pasch by Italian mathematicians and Hilbert. Russell also gave an informal presentation in The principles of mathematics. A rational reconstruction of Hertz's system of Euclidean geometry had still to be given [27].

In the second place, Hertz's system required material particles - Massenteilchen, Massen - either in motion (occupying a continuous series of places at a continuous series of times) or in rest (occupying one place at a continuous series of times). There is no independent theory of mass [28]. The problem of characterizing the structure of a system of material particles was posed by Russell, again in The principles of mathematics. He gave an informal statement of characteristic properties of "material units" in such a way that a formalization could easily be given. This can be seen as a first contribution to a rational reconstruction of a part of Hertz's system. As a matter of fact, Russell dealt with Hertz's "Dynamics" in the last chapter of The principles, interested as he was as a pure mathematician in this theory in so far as it defined a certain kind of matter. (Cf. Russell 1903a, section 459.) His aim was a characterization of the mathematical structure of Hertz's theory. He was explicitly not concerned with the truth or falsehood of laws of motion in relation to the actual world. But he must have seen that Hertz's elimination of a concept of force or energy was in accordance with the current view of the physicists of his time that "force is a mathematical fiction, not a physical entity" (Russell 1903a, sect. 455; cf. sect. 470). Only Whitehead drew general consequences on this point in Part I (i) "General considerations" of his memoir "On mathematical concepts of the material world". His program of ontological reconstructionism will be dealt with in the following chapter.

CHAPTER FIVE

WHITEHEAD'S PROGRAM OF FORMAL ONTOLOGICAL RECONSTRUCTIONISM

Introduction

"The object of this memoir is to investigate the mathematical investigation of various possible ways of conceiving the nature of the material world."

The opening sentence of Whitehead's paper "On mathematical concepts of the material world", read before the Royal Society in London in 1905, announces nothing less than a new field for mathematical research. It sounds like an answer to Hilbert's sixth "problem" of the mathematical treatment of the axioms of physics, but Whitehead's results were given in a symbolism few people were familiar with at the time of its publication. This may explain why the memoir had such little impact on the scientific world: neither mathematicians nor physicists seem to have shown interest in the results he reached [29]. This is not to say that Whitehead's contribution was without any influence at all. In my opinion, the general considerations became crucial for Russell's program of scientific philosophy of 1914. Moreover, Whitehead's own later philosophical work was also closely connected with the content of the memoir. Given the influence of Russell's book of 1914, notably on Carnap, and the influence of Whitehead's books of 1919 and 1920 on Lesniewski and Tarski, we may say that "On mathematical concepts of the material world" was indeed of considerable importance [30].

Starting point of the memoir is the insight that there are different ways of giving an axiomatic theory about "the material world" conceived as "a set of relations and of entities which occur as forming the fields of these relations" (Whitehead 1906a; 1953a, p. 13). This standpoint combined several ideas: (1) the possibility of different axiom systems for (projective) geometry, supported by Pasch and Italian mathematicians; (2) the possibility of different theories of mechanics, elaborated by Hertz and Russell; (3) a characterization of different (philosophical) theories of space and time, worked out by Russell in terms of "basic entities" and relations between them. Whitehead realized that each such an axiomatic theory, called a "concept", represents a particular view about the material world, reflected in the assumption of (what he called) the "fundamental relations" and the "ultimate existents". This means, in my terminology, that Whitehead considered at least theoretically the possibility of making different formal ontological reconstructions explicit. In practice, he realized only part of the program, by restricting his analyses to the special problem of reconstructing a Euclidean theory of space within the framework of such ontological reconstructions: "In so far as its results are worked out in precise mathematical detail, the memoir is concerned with the possible relations to space of the ultimate entities which (in ordinary language) constitute the 'stuff' in space" (Whitehead 1906a, 1953a, p. 11). This limited problem, however, did not prevent Whitehead from reflecting on the nature of his investigations and the method of procedure. He not only developed a precise notion of formal ontological reconstruction, but also presented a formal apparatus for such reconstructions. It is true that he was "not concerned with upholding or combatting any theory of the material world", but nevertheless he did not leave untouched the question of the co-equality of different axiom-systems - die Frage nach der Gleichberechtigung verschiedener Axiomensysteme - , as Hilbert called it. Whitehead suggested criteria for preferring one reconstruction above another. These will be dealt with in a seperate section after two sections on Whitehead's proper reconstructions and formal apparatus.

Whitehead's reconstructions

From a meta-physical point of view, classical mechanics presents a perspicuous picture of the material world: it contains, apart from the classical theory of time, a "classical theory of space", which assumes geometrical axioms for the so-called points of space; the fundamental "stuff" consists of so-called material particles, each of which is said to "occupy" exactly one point of space at each instant of time, whereas no two particles occupy the same point of space at the same instant of time. Finally, the physical laws are nothing other than properties of this relation of occupation. According to weak formal ontological reconstructionism, a straightforward formalization of classical mechanics would indeed reflect the ontological position that there are three mutually exclusive classes of "ultimate existents", instants of time, points of space, and material particles. The world view just-sketched might satisfy philosophers, but they may also want to know whether there are alternatives to this conception of the physical world called the Classical Concept of the material world by Whitehead. For example, is it possible to give a formal ontological reconstruction of classical mechanics in such a way that no points of space are assumed to exist, thereby presenting a modern version of Leibniz's theory of the relativity of space? Such questions are notably what Whitehead wanted to answer in his memoir "On mathematical concepts of the material world". As a first step, he dealt with the problem of reconstructing the Euclidean theory of space within different ontological frameworks. This meant for the Classical Concept of the material world that there had to be axioms for a relation R having points of space as its members, such that "the whole of Euclidean geometry" could be deduced from them (cf. Whitehead 1906a, 1953a, p. 28).

In order to get other "Concepts" - let me adopt Whitehead's terminology for the moment - there must be axioms for a relation R not having points of space as its members, but, as a Leibnizian might suggest, material particles and instants of time. That is, Whitehead wanted to solve the following problem for special cases of the relation R: "Given a set of entities which form the field of a certain polyadic (i.e., many-termed) relation R, what 'axioms' satisfied by R have as their consequences that the theorems of Euclidean geometry are the expression of certain properties of the field of R?" (Whitehead 1906a; 1953a, p. 11). Whitehead had no analogous problem for the theory of time, since he concluded from Russell's article "Is position in time and space absolute or relative?" that the material world could be conceived only as having instants of time among its ultimate existents.

Different concepts of the material world can now be compared with respect to the classes of those ultimate existents which are not instants of time, or, as Whitehead called them, "objective reals". Thus, Whitehead distinguished between "punctual" and "linear" Concepts, accordingly as the objective reals have a point-like or a line-like character. He also distinguished between "dualistic" and "monistic" Concepts, accordingly as there are two mutually exclusive classes of objective reals, or only one single class. Clearly the Classical Concept is a punctual and dualistic Concept. Straight lines and planes are so-called defined entities; they appear as classes of points.

We have seen that Whitehead wanted to accomplish that in every Concept all the propositions of Euclidean geometry were exhibited as properties of a single polyadic relation. Such a relation was called the "essential relation" and had to be distinguished from the so-called extraneous relations needed for the formulation of physical laws. In the Classical Concept, the essential relation is Pasch's betweenness relation in which three points stand as soon as they are in a linear order (Whitehead 1906a, p. 25); it is possible to assume one extraneous relation of occupation, in which a particle, a point and an instant can stand (Whitehead, o.c., p. 29).

An example of a linear Concept is a "dualistic" Concept according to which the class of objective reals is composed of two mutually exclusive classes of entities, lines of force and particles. In this Concept, points are defined entities, namely classes of lines of force. How this is possible will appear from the following résumé of Whitehead's reconstructions, which vary from the Classical punctual and dualistic Concept, to a "modern" linear and monistic Concept.

In the Classical Concept, the essential relation is already a relation between points of space. This made Whitehead's task easy: as soon as the axioms of Euclidean geometry are formulated in terms of this relation, the limited problem has been solved. Whitehead achieved this with reconstructions of geometrical concepts such as "straight line", "triangle" and "plane", together with twelve axioms, borrowed from Veblen's memoir "A system of axioms for geometry". The obtained system was considered "the" theory of space which Whitehead wanted to reconstruct within other Concepts of the material world.

A simple example is a Concept which acknowledges "particles of ether" (or moving points) as objective reals. Here, Whitehead took for the essential relation a quaternary relation in which three objective reals a, b, c, and an instant of time t stand as soon as a, b, c are in the order abc at the instant t. The difference with the foregoing Classical Concept is not very great as far as the geometrical theory is concerned: at each instant the objective reals are treated as the points of the classical concept.

The not purely geometrical physical sides of the foregoing Concepts were only sketched by Whitehead. As for the Classical Concept, he followed Russell's earlier discussion of a characterization of matter. Whitehead pointed out that there are at least two alternatives for the external relations: either specific relations of occupation are chosen, one for each particle in connection with points of space and instants of time, or a general relation of occupation is introduced holding between particles, points of space and instants of time. In the first case, the so-called "impenetrability of matter is secured by the axiom that two different extraneous relations cannot both relate the same instant of time to the same point"; in the second case, this is done by the axiom that there is at most one particle occupying any given point of space and instant of time. This subject will be discussed in the third section; it will appear that Whitehead did not eschew philosophical discussion.

As for the Concept with moving points, Whitehead indicated that it could dispense with material particles as soon as (1) an axiom of persistence has been added, and (2) provisions are made that a point at one instant can be said to have the same position as a point at another instant (Whitehead 1906, p. 31). The first condition can be stated in terms of the essential relation, the second in terms of a single extraneous relation holding between instants of time and so-called kinetic axes. What is important in all this, is that the resulting concept can indeed be considered a reconstruction of a certain world view (Whitehead 1906, p. 31):

> The concept pledges itself to explain the physical world by the aid of motion only. It was indeed a dictum with some eminent physicists of the nineteenth century that 'motion is of the essence of matter'. But this concept takes them rather sharply at their word. There is absolutely nothing to distinguish one part of the objective reals from another part except differences of motion. The 'corpuscle' will be a volume in which some peculiarity of the motion of the objective reals exists and persists.

The foregoing Concepts were examples of punctual Concepts. Reconstruction of "the" theory of space within linear Concepts was more complicated. But the project was interesting enough, given the fact that linear Concepts were also intended to be reconstructions of world views. As Whitehead said, the linear objective reals of these Concepts "are the lines of force of the modern physicist, here taken to be ultimate unanalysable entities which compose the material universe". In order to solve Whitehead's limited problem, the first thing to do was to define points in terms of linear objective reals. Intuitively, a point had to be defined in some way as the class of objective reals "concurrent" at a point. Such a definition could indeed be given without circularity, but only after several ideas were brought together. Second, it had to be stated when three such defined (so-called) "interpoints" A,B,C are in the order ABC. Whitehead showed that this could also be done [31]. The way was thereby paved for the formulation of Euclidean geometry within linear Concepts. One proceeds just as with punctual Concepts, at least as far as the geometrical part is concerned. The matter is more complicated for the "material side", because the linear objective reals are considered lines of force. For if particles are accepted as objective reals beside the linear ones, each particle can be associated at each instant with some (defined) point, whereas laws of motion must be stated for particles and for the linear objective reals. Moreover, as Whitehead remarked, "the motion of the particles may be conceived to be influenced by that of linear objective reals, and vice versa". Nevertheless, Whithehead was convinced that a reconstruction of contemporary electro-magnetic theory could be carried out within such a dualistic framework; he thought that it was possible to formulate the required laws of motion (Whitehead 1906, p. 43):

> The endeavour to state such laws appears to reduce itself to rewriting with appropriate changes a chapter of any modern treatise of electricity and magnetism. It would seem necessary to subdivide the class of particles into 'positive' and 'negative' particles, a charged volume containing an excess of one type. The conception of an ether conveying lines of force is replaced by the class of the linear objective reals. The details can be managed much as in the analogous case of Concept V., considered later.

Concept V was a monistic linear Concept for which Whitehead's expectations were greater than for the other Concepts. Already in the preface of the Memoir, he remarked that Concept V in particular appeared to have great physical possibilities. This Concept made use of the theory of interpoints, as well as a so-called theory of dimensions. Its exposition would far exceed my present purpose. What makes things so complicated here, is that in this Concept, geometry has not only to do with geometrical points, lines and planes, but also with linear objective reals and interpoints. The difference between geometrical points and interpoints - identified with negative "electric points" - was reflected by a difference in definitional procedures. Whitehead invented a "theory of dimensions" in order to reconstruct geometrical straight lines and geometrical points (as classes of straight lines) [32].

In my view, the significance of Whitehead's reconstructions for the development of formal ontological reconstructionism rests mainly on the following results of the memoir "On mathematical concepts of the material world":

(1) the development of a precise notion of formal ontological reconstruction
(2) the presentation of a formal apparatus in which ontological reconstructions can be carried out
(3) the indication of criteria for the preference of one reconstruction above another.

The first result was dealt with in the foregoing pages; the second will be treated in the next section; the chapter closes with a section on "criteria".

Of course, these three results strongly cohere: the notion of ontological reconstruction depends on the distinction between fundamental relations and the members of their fields on the one hand, and defined entities and relations between these on the other, as shown in the technical elaboration of the reconstructions. The motivation behind a particular reconstruction may be external, as Whitehead suggested in the case of "Leibnizian Concepts" (Whitehead, o.c., p. 14-15). That the heart of the enterprise is ontological, could be formulated in the principle that only the ultimate existents have ontological status, whereas defined entities do not. Whitehead's distinction between monistic and dualistic Concepts seems to conform to this principle; as a matter of fact, we have his own comment on the situation of Concept V - in which the only (kind of) ultimate existents are lines of force - : "In this case particles (...) do not exist. Corpuscles, to use another term, are defined entities, analogous to the corpuscles of Concept III" (Whitehead, o.c., p. 33).

But before one can speak of "defined entities", exact definitions have to be given. Clearly the possibilities on this point are determined by the possibilities of the formal apparatus. In the example just given, particles were considered classes of ultimate existents; therefore Whithehead's formal apparatus contained representations of classes, together with a theory for handling them in such a way that a given theory about, say, particles can be formulated in terms of classes. So I now turn to a short account of Whitehead's formal apparatus.

The formal apparatus

Whitehead's reconstructions could in principle have been given in the language of mathematics of that time. However, in the informal account for the procedure of defining interpoints, so many variables were employed that the choice of a general symbolism would be helpful, if only by way of shorthand. Moreover, in a number of cases, informal rigour might challenge comprehensibility. Arguments like these may have played a role when Whitehead gave the following comment (Whitehead 1906, p. 18-19):

> None of the reasoning of the paper is based upon any peculiarity of the symbolism. It is used here only as an alternative form for enunciations, for the sake of its conciseness and (above all) precision. In the verbal enunciations precision has been to some extent sacrified to lucidity; and the exact statement of what is meant is always to be sought in the symbolic alternative form. The proofs have been translated into words out of the symbolic form in which they were mostly elaborated.

However, practically nowhere in Whitehead's article is the logic which is used in the proofs discussed; only the symbolism is explained. Indeed, its main function seems to be that the reconstructions can be formulated in sentences of a syntactically precise form. This demanded a higher order predicate language including class abstraction operators and class variables. It enabled Whitehead to symbolize classes of entities which are not classes, and classes of classes. This might seem premature: was the underlying theory of classes justified? How could Whitehead state that none of the reasoning of his paper was based upon any peculiarity of the symbolism, in a time that Russell's paradox had not yet been solved?

A possible answer is that Whitehead believed that his use of classes was uncontroversial, since he was not concerned with what he preferred to call extreme cases; but there is also the suggestion that the logical analysis of propositions with class-expressions was not as such a task for a reconstructionist (Whitehead, o.c. p. 117-18):

> None of the reasoning of this memoir depends on any special logical doctrine which may appear to be assumed in the form in which it is set out. Furthermore certain contradictions recently discovered have thrown grave doubt upon the current doctrine of classes as entities. Any recasting of our logical ideas upon the subject of classes must of course simply issue in change of our ideas as to the true logical analysis of propositions in which classes appear. The propositions themselves, except a few extreme instances which lead to contradictions, must be left intact. Accordingly the present memoir in no way depends upon any theory of classes.

Let me explain. To begin with, it can be asked what Whitehead could have meant with "the true logical analysis of propositions in which classes appear". One is reminded of Russell's attempts to make a theory of classes in which his and Burali-Forti's paradox did not occur. In his paper "On some difficulties in the theory of transfinite numbers and order types", Russell concluded that the contradictions show that "a propositional function of one variable does not always determine a class". He considered two possibilities: (1) "we may decide that all ordinary straight-forward propositional functions of one variable determine classes and what is needed is some principle by which we can exclude the complicated cases in which there is no class", (2) "that there are no such things as classes and relations and functions as entities, and that the habit of talking of them is merely a convenient abbreviation" in the sense that every ordinary proposition in which classes - determined by a propositional function - occur, can be expressed by a statement about the separate values of the propositional function in question. If Whitehead had something like (2) in mind when he wrote the above passage, then I gather from what he said that no logical theory of classes in Russell's sense would overthrow his reconstructions. When points are defined as classes of lines of force, ordinarily or mathematically spoken they remain classes and therefore defined entities which do not exist. If, on the other hand, Whitehead accepted possibility (1), in which classes are entities in a logical sense, then reconstruing points as classes of lines of force does not make them entities in an ontological sense (or existents). Nothing is changed if one adheres to a logical theory in which classes are not entities in a logical sense; the points in question remain defined entities which as such do not exist.

This is not to say that logical theories are not important for ontological reconstructions: they serve to make precise the findings of the reconstructionist procedures. If such procedures employ classes which have to play the role of certain entities (such as points), then logical theory must systematise or refine these procedures in such a way that sentences about points can be reformulated into sentences of a syntactically precise form in terms of classes and that exact proofs become possible. It is indeed thinkable that such sentences can be further logically analyzed, say in terms of propositional functions, but that makes no difference for the given reconstructions. The conclusion thus seems warranted that the logical analysis of classes is not the reconstructionist's business.

The situation is the same for so-called definite descriptions. These are also part of Whitehead's symbolism (Whitehead 1906a, p. 20). At that time, Russell had just developed his logical theory according to which every sentence of the form the so-and-so is a such-and-so can be logically analyzed in terms without expressions of the form the so-and-so. Suppose now that we want to reconstruct a theory which assumes a unique point somewhere, say the point at which the three medians of a Euclidean triangle meet each other. With a symbol of the form $(\iota x)(\varphi x)$ we can directly name this point formally. To be true, the very idea of formal reconstructionism demands such a formal counterpart of an informal definite description. But this has nothing to do with the well-known question of non-referring definite descriptions in sentences such as 'the largest prime number does not exist' and 'the present king of France does not exist'. Russell dealt with this problem just before Whitehead read his memoir. Whitehead referred to this article when he commented on the statement that "if u is not a class, there is no such entity as its cardinal number" (Whitehead 1906a, p. 22, footnote):

> The difficult question of the import of a proposition, which contains a non-propositional function (with some particular entity as argument) to which no entity corresponds, has recently been elucidated by Russell, cf. Mind, October, 1905. All propositions containing such a function are untrue, unless the function is merely a constituent of a subsidiary proposition whose truth is not implied by the proposition in question.

In my opinion, this quotation puts the problem in the proper light, by treating it as a purely logical problem. It is true that one can take a different view by considering a solution to this problem also relevant for non-logical questions. It is well-known that Russell in "On denoting" already made use of his solution in the theory of knowledge (see Part Three). But such applications are completely absent in Whitehead's memoir.

The above considerations indicate that for Whitehead the function of a symbolism in the context of ontological reconstructions was to make exact formulations of Concepts of the material world possible. Problems within the logical theory behind the symbolism had no bearing upon the tenability of a Concept. As a matter of fact, questions of acceptability of Concepts had to be decided on other than logical grounds. This is the subject of the next section.

Criteria

In the Preface to his memoir "On mathematical concepts of the material world", Whitehead wrote that the general problem of finding various formulations for Euclidean geometry was "discussed purely for the sake of its logical (i.e. mathematical) interest"; it was said to have (only) "an indirect bearing on philosophy by disentangling the essentials of the idea of a material world from the accidents of one particular concept" (Whitehead, o.c., p. 11-12). I have quoted Whitehead's remark that he was "not concerned with upholding or combatting any theory of the material world". Nevertheless, he showed a certain preference for his Concept V, because he believed that it was well suited to incorporate modern physical ideas (cf. Whitehead, o.c., p. 60, p. 81-82). But he gave no philosophical arguments for his preference, so that we cannot attach much weight to Passmore's diagnosis that "one can see in this Memoir why Whitehead was dissatisfied with 'the classical concept of the material world' and what kind of ontology he hoped to substitute for it" (Passmore 1957a; 1968a, p. 337). The reason why I call Whitehead's approach weak formal ontological reconstructionism is just that he did not defend a particular ontological reconstruction. His sole purpose was "to exhibit concepts not inconsistent with some, if not all, of the limited number of propositions at present believed to be true concerning our sense-perceptions" (Whitehead o.c., p. 14).

Yet Whitehead did indicate on several occasions the kind of reasons that could be given for preferring one particular reconstruction to another. In my opinion, he thereby indirectly showed that different reconstructions reflect different ontologies which might be defended with philosophical arguments, even though this was not his concern in the memoir. When Whitehead called the class of members of the field of fundamental relations the class of ultimate existents, he said that he adopted this "technical name" without prejudice to any philosophic solution of what he called "the question of the true relation to existence of the material world as thus conceived". Here, the use of the phrase 'relation to existence' indicates that the notion of "ultimate existents" has an ontological character. It follows that the same holds for the distinction between monistic and dualistic concepts (o.c., p. 15):

> Definition. - Any concept of the material world which demands two classes of objective reals will be called a Dualistic concept; whereas a concept which demands only one such class will be called a Monistic concept.

It is here that the question of criteria for the preference of one concept above another becomes important (o.c.):

> Occam's razor - Entia non multiplicanda praeter necessitatem - formulates an instinctive preference for a monistic as against a dualistic concept.

As we shall see, a variant of this kind of criterion played an important role in the rationale for the choice of a particular formal ontological reconstruction. In 1914, Russell considered Occam's razor "the maxim which inspires all scientific philosophizing", giving it the following interpretation (Russell 1914a, p. 107):

> Entities are not to be multiplied without necessity. In other words, in dealing with any subject matter, find out what entities are undeniably involved, and state everything in terms of these entities. Very often the resulting statement is more complicated and difficult than one which, like common

> sense and most philosophy, assumes hypothetical entities whose existence there is no good reason to believe in.

Formulated this way, the principle is not merely a methodological criterion of economy; it is used to dispense with entities whose existence is considered difficult to defend, because for example they cannot be found in experience. If an argument like this is given, the ontological reconstruction in question reflects one's philosophical position: it establishes an ontological view supported by epistemological considerations.

The just-mentioned use of Occam's razor is not wholly absent in Whitehead's Memoir. This appears from a discussion of a variant of the Classical Concept suggested by Russell in The principles of mathematics. We have seen that Russell in par. 440 of this work sought for a characterization of matter; also that he "replaced" material points by relations in order to get an "abstract logical statement of the subject matter of Rational Dynamics" in par. 441. Russell's argument for this was that "it is plain that the only relevant function of a material point is to establish a correlation between all moments of time and some points of space, and that this correlation is many-one". Whitehead took this over from Russell (Whitehead o.c., p. 29):

> In the classical concept the particles only occur as terms in the triadic extraneous relations. If we abolish the particles (in the 'classical' sense), and transform the extraneous relations into dyadic relations between points of space and instants of time, everything will proceed exactly as in the classical concept.

The result was a monistic variant of the Classical Concept, and a simple application of Occam's razor would involve a preference for this variant above the Classical Concept itself. But other considerations are possible, as Whitehead remarked (Whitehead o.c., p. 29-20):

> The reason for the original introduction of 'matter' was, without doubt, to give the senses something to perceive. If a relation can be perceived, this Concept II has every advantage over the classical concept. Otherwise the material world, as thus conceived, would appear to labour under the defect that it can never be perceived. But this is a philosophic question with which we have no concern.

Here there is at least mention of the possibility that a discussion of a certain Concept deals with the question whether the Concept can or cannot be accepted for philosophical reasons. This gives a distinction between philosophically acceptable and philosophically inacceptable Concepts which does not coincide with the first distinction between monistic and dualistic Concepts. In the above example, there was a doubt as to the perceivability of fundamental relations of a Concept. But this is not the only philosophical way of looking at a reconstruction, as we can see from a discussion of linear Concepts, given by Whitehead shortly after some remarks on the problem of the alleged circularity of the definition of a point (Whitehead o.c., p. 33):

> More difficulty will probably be felt in conceiving anything analogous to a line as a simple unity. Here it is to be observed that a linear objective real does not replace a line of points of ordinary geometry. On the contrary, the class of those points (here called a punctual line), which have a given linear objective real as a common member, is this ordinary geometrical line. A punctual line has parts and segments in the ordinary way. The idea of a single unity underlying a straight line is not wholly alien to ordinary language. The idea of a direction, as it could also be used in non-Euclidean geometries where each line will have its own peculiar direction, may be conceived as being that of a line taken as a unit. But it is unnecessary to elaborate these

considerations, as they have no relation to the logic of the subject.

A philosopher who expresses his doubts as to the possibility of ultimate existents on the ground that lines are not "single unities", seems to hold an ontological simplicity criterion: what exists is "simple" in some philosophically relevant sense of the word 'simple'. Apparently, Whitehead did not reject this criterion in his defense of the acceptance of the intended linear objective reals. On the contrary, he argued that such things can indeed be seen as single unities. Whether his argumentation follows a viable philosophical line of thought is not in question here; <u>what matters is that the basic assumptions of a certain Concept or reconstruction make an ontological view explicit</u>. Or, as Whitehead put it, a Concept involves the assumption of classes of "entities as forming the universe" (o.c., p. 82). This makes Whitehead's formal reconstructionism more than a mathematical exercise - hence my calling it formal <u>ontological</u> reconstructionism. What is missing in Whitehead's memoir is an elaborate argument for a particular reconstruction. Therefore Whitehead's formal ontological reconstructionism cannot be labeled as a philosophical doctrine.

It was Russell who recognized the significance of the reconstructionist approach for philosophy. In his book <u>Our knowledge of the external world</u> of 1914, he called it <u>the scientific method in philosophy</u>. In so far as Russell's contributions are a confirmation of Whitehead's ideas in the <u>memoir</u> "On mathematical concepts of the material world", a treatment of these contributions seems now to be in order.

Whitehead also kept working in this field. Another contribution, "La théorie relationniste de l'espace" was written in 1914 and read in Paris at a congress in May of that year.

CHAPTER SIX

RUSSELL'S CONTRIBUTIONS TO ONTOLOGICAL RECONSTRUCTIONISM

Introduction

When Whitehead wrote his memoir "On mathematical concepts of the material world", he didn't intend to do philosophy. His aim was rather "to initiate the mathematical investigation of various possible ways of conceiving the nature of the material world". Given the highly technical character of the paper, there is reason to assume that most philosophers of that time would see it as nothing other than a mathematical study with no bearing on philosophy. Only Russell realized after a while that Whitehead's approach could give philosophy a new impulse and bring it to a more scientific level. Earlier he considered philosophy as a kind of discursive activity. His paradigm was G. E. Moore, whose influence is manifest in The problems of philosophy (Russell 1912a). That the transition from a Moorean approach in philosophy to formal ontological reconstructionism was nevertheless not so difficult, may be attributed to Moore's preoccupation with ontological issues.

In this section, I sketch how Russell's contributions to ontological reconstructionism arose and try to isolate some problems which formal ontological reconstructionism has to face. In the introduction, I discuss some elements of Russell's philosophical position as found in The problems of philosophy. Questions on the general nature of doing philosophy will be left aside however. (Russell also followed Moore in stating that philosophical questions are in a sense more important than philosophical answers; recall that Moore, in Principia ethica, demanded that philosophers should try first "to discover what question they were asking" before setting about to answer it.)

Russell did not immediately see how Whitehead's ideas in the above-mentioned memoir could change the whole outlook of philosophy.

According to Moore, "the most important and interesting thing which philosophers have tried to do" is "to give a general description of the whole of the Universe, mentioning all the most important kinds of things we know to be in it, and also considering the most important ways in which these various kinds of things are related to one another" (Moore 1953a, p. 1). It is true that Moore's conception of philosophy shared with Whitehead's treatment a concern with world views, but there are conspicuous differences. Whitehead studied scientific theories of the material world, Moore considered philosophical views. Whitehead accomplished his task by a rigorous reconstruction of the theories in question in a formal language, Moore tried to reformulate philosophical views in English. Nevertheless shortly after The problems of philosophy, Russell saw a possibility of applying Whitehead's method to Moorean problems. Some insight into Moore's and Russell's philosophical positions in the early 1910's is therefore helpful in understanding Russell's "new start" in 1914.

One of Moore's main problems of philosophy was the epistemological question: Do we, any of us, ever know of the existence of any material object, that is, "something which (1) does occupy space; (2) is not a sense-datum of any kind whatsoever and (3) is not a mind, nor an act of consciousness"? (Moore 1953a, p. 131).) His method consisted partly of establishing conclusions from "experiments", for example (Moore 1953a, p. 30):

> I hold up this envelope, then: I look at it, and I hope you all will look at it. And now I put it down again. Now what has happened?

By way of example, and also as a background for Russell's related discussion, I repeat two of Moore's conclusions from this demonstration (Moore 1953a, p. 32; p. 33):

(M1) Though we all did (as we should say) see the same envelope, no two of us, in all probability, saw exactly the same sense data.

(M2) .. _if_ we _did_ all see the same envelope, the envelope which we saw was not _identical with_ the sense-data which we saw: the envelope cannot be exactly the same thing as each of the sets of sense-data, which we each of us saw ...

As one knows, Moore distinguished two ways of knowing, "direct apprehension" (of sense-data and images, and also of acts of consciousness) and other ways of knowing in which we are directly apprehending propositions about, among other things, material objects, without directly apprehending these objects themselves. In this case Moore speaks of "indirect apprehension" (Moore 1953a, p. 85):

(M3) That you do, when you directly apprehend certain sense-data, _often_ thus believe in the existence of someting else is, I think, certain. And, if this something else _is_ a material object, then you really are, whenever you do it, indirectly apprehending a material object.

Shortly after Moore's lectures, Russell got the opportunity to write a volume on philosophy in a series of introductory texts called "Home University Library". He did not conceal that he "derived valuable assistance from unpublished writings of Mr. G. E. Moore" as regards the relations of sense-data to physical objects (Russell 1912a, p. v). It is for our purpose not necessary to thrash out the differences between Moore's and Russell's treatment; on the whole, Russell's approach seems to be more ontological than Moore's.

Russell started from the following analogues of Moore's conclusions (Russell 1912a, p. 15-17):

(R1) ... a given thing looks different in shape from every point of view.

(R2) ... the real table, if there is one, is not the same as what we immediately experience by sight or touch or hearing.

(R3) The real table, if there is one, is not <u>immediately</u> known to us at all, but must be an inference from what is immediately known.

In this connection it is interesting to notice that Russell appealed only to different sense-data of one person in order to reach the first conclusion, whereas Moore started from sense-data of different persons. Russell had reason for that: an appeal to other people as an argument for the existence of objects independent of our sense-data would beg the question. Russell also used explicitly the Cartesian method of doubt, whereas Moore mainly discussed and tried to refute deviations from what he called common sense view.

Ontological questions were posed immediately after the last quotation (Russell 1912a, p. 17):

> Hence, two very difficult questions at once arise; namely, (1) Is there a real table at all? (2) If so, what sort of object can it be?

More generally, Russell asked "Is there any such thing as matter?" and "If so, what is its nature?" (Russell 1912a, p. 18). He answered the first question by: "every principle of simplicity urges us to adopt the natural view, that there really are objects other than ourselves and our sense-data which have an existence not dependent upon our perceiving them" (Russell 1912a, p. 37). In other words, the hypothesis that there are "physical objects", as Russell called such objects, gives a simpler means of "accounting for the facts of our life" than the solipsistic hypothesis that the world consists only of myself and my thoughts and feelings and sensations. (Think of the fact that "one person in a given place at different times has similar sense-data" (Russell 1912a, p. 33).)

Accordingly, Russell's ontology in The problems of philosophy comprised not only sense-data, private spaces and private time, but also "inferred entities" such as physical objects, physical space and physical time. Concerning the nature of physical objects, Russell mentioned physical science and its view that "all natural phenomena ought to be reduced to motions". "That which has the wave-motion is either aether or "gross matter", but in either case is what the philosopher would call matter", he said, in a terminology which came very close to Whitehead's formulations in his memoir (cf. Whitehead 1906a, p. 33-34). The philosophical relevance of physical theories was, for Russell, mainly that they justified a distinction between physical space in which the wave-motions take place and people's "private spaces" in which sense-data are situated. He assumed that the spatial relations which physical objects have in physical space "correspond" to those which the corresponding sense-data have in private spaces. A similar assumption was made for physical time. Russell also accepted a correspondence between qualities in the physical objects and properties of sense-data, for example, if one object looks blue and another red (or blue) then there is some corresponding difference (or similarity) between the physical objects.

Russell had also something to say on "knowledge" of physical objects, given the fact that we are not "acquainted" with such things. His well-known distinction between (knowledge by) acquaintance and "knowledge by description" can be seen as a specification of Moore's distinction between direct apprehension and indirect apprehension.

This short account of part of Russell's philosophical position in The problems of philosophy concludes my introduction to the section on Russell's reconstructionism. Perhaps it is too much to say that this position is to be located between Moore and Whitehead, but so much is certain that Russell took physical world views seriously and scrutinized explicitly the question of the relation of sense-data to physical objects. Russell's later work in formal ontological reconstructionism was specifically concerned with this relation between "the crude data of sense" and the space, time and matter of mathematical physics. The significance of the above discussion is that the relevant chapters of Our knowledge of the external world as a field for scientific method in philosophy include a reconsideration of the first three chapters of The problems of philosophy.

Russell's reconstructions; general considerations

Formal ontological reconstructionism as a kind of philosophy begins with Russell's "logical reconstructions" as a means to solve philosophical problems. This idea was so central for him, that he made it a cornerstone of a new kind of "scientific philosophy", representing "the same kind of advance as was introduced into physics by Galileo: the substitution of piecemeal, detailed, and verifiable results for large untested generalities recommended only by a certain appeal to imagination" (Russell 1914a, p. 4). The first problem which Russell broached was not a modest one however: "is the inference from sense to physics a valid one?" Nor was his answer to this question an example of piecemeal engineering. In fact, he gave no more than a rough sketch of a solution, as he himself was aware when he acknowledged his debt to Whitehead (Russell 1914a, p. vi):

> I owe to him the definition of points, the suggestion for the treatment of instants and "things", and the whole conception of the world of physics as a construction rather than an inference. What is said on these topics here is, in fact, a rough preliminary account of the more precise results which he is giving in the fourth volume of our Principia Mathematica.

There is a notable exception: Russell's own reconstruction of the physico-mathematical time-series. This is formal ontological reconstructionism at its best [33].

In this section, I deal primarily with Russell's reconstruction of "things" in so far as it fulfils the general conditions of his reconstructionism. Two observations play an important role in the reconstruction:

(1) a general presupposition: there is a distinction between psychologically primitive and derivative beliefs,
(2) a basic assumption how to do scientific philosophy: wherever possible, logical constructions are to be substituted for inferred entities.

To these observations, Russell added the methodological qualification that his resultant reconstructions are hypothetical. I shall return to this aspect.

Let us first envisage the consequences of the above two observations. The first general assumption linked Russell's project with the empiricist tradition and provides it with a fruitful starting point. For it presented Russell with a criterion to determine which kinds of entities are acceptable as basic. The idea was that psychologically primitive beliefs, for which "no further argument is required" are about "hard data", "which resist the solvent influence of critical reflection". These hard data were not only the facts of sense - our own sense-data - and the laws of logic, but also facts of recent memory and introspective facts, in so far as they have "the highest degree of certainty" (Russell 1914a, p. 72). Apart from the laws of logic, there is not much difference with the kinds of things we are acquainted with according to The problems of philosophy. (Again, Russell did not realize that acquaintance by memory, even recent memory, is different from acquaintance with sense-data: we are directly acquainted with the memory impression as such, not with the sense-data which we believe to correspond to it.) But Russell added some examples of how "facts of sense themselves must, for our present purposes, be interpreted with a certain latitude" (o.c.):

> Spatial and temporal relations must sometimes be included, for example in the case of a swift motion falling wholly within the specious present. And some facts of comparison, such as the likeness or unlikeness of two shades of colour, are certainly to be included among hard data.

Russell appealed to these data in the course of his argument. In sharp contrast with the hard data, there are the contents of psychologically derivative beliefs, such as the belief in the permanence of the external world. Russell already concluded in The problems of philosophy that physical objects are not things with which we are acquainted. Therefore physical objects, as opposed to sense-data, might only be obtained by an inference (cf. Russell 1912a, p. 170). In Our knowledge of the external world Russell asked, in his new terminology, whether the existence of anything other than our hard data can indeed be inferred from (the existence of) those data. He discussed this at length in Lecture 3. The following is a short account of this discussion and its outcome.

Is it necessary to assume material objects? The common-sense view that there are such things seems plausible, since it yields a simple explanation for changes in sense-data: for example, if we put on blue spectacles, then the changes in the appearance of things can be said to be changes in the intervening medium, whereas the material objects themselves remain the same. Russell however argued that we can completely account for what happens here in terms of actual sense-data alone, that is "without assuming anything beyond the existence of sensible objects at the times when they are sensible" (Russell 1914a, p. 80):

> By experience of the correlation of touch and sight sensations, we become able to associate a certain place in touch-space with a certain corresponding place in sight-space.

With the help of arguments of this kind - the validity of which does not concern us - Russell reached a position quite similar to that of Mach in Die Analyse der Empfindungen: the rejection of the assumption of permanent "things" with changes in "appearances". But Mach was not a "formal ontological reconstructionist". He was content with the formulation that what we call a material object - Körper - is nothing other than a "complex" of functional dependent sensations: "Nicht die Körper erzeugen Empfindungen, sondern Elementarkomplexe (Empfindungskomplexe) bilden die Körper". It is not that Russell disagreed with this view, but he realized that there was a technical problem (Russell 1914b, (1963a), p. 108-109):

> We may succeed in actually defining the objects of physics as functions of sense-data. Just in so far as physics leads to expectations, this must be possible, since we can only expect what can be experienced. And in so far as the physical state of affairs is inferred from sense-data, it must be capable of expression as a function of sense-data. The problem of accomplishing this expression leads to much interesting logico-mathematical work.

In other words, Russell was not satisfied with merely verbal characterizations of the "constructs" of Mach and Pearson, he wanted to substitute the supposed inferred entities by a "logical function of less hypothetical entities" (Russell 1914b, (1963a), p. 116). This is a consequence of the above-mentioned general claim that "wherever possible, logical constructions are to be substituted for inferred entities" (o.c., p. 115).

Russell emphasizes the difference between traditional approaches and the application of mathematical logic in Our knowledge of the external world. He distinguished views of physicists, metaphysicians and psychologists, apparently considering all unsatisfactory: physicists thought that in practice they could assume particles, points, and instants without "claim to metaphysical reality"; metaphysicians regarded the notions of matter, space and time contradictory, while psychologists called them "intellectual constructions", but they did not attempt "to show in detail either how the intellect can construct them, or what secures the practical validity which physics shows them to possess" (o.c., p. 123). Russell's answer to the physicists was that the assumption or "convenient fiction" that there are "points" and "instants" is indeed logically possible and consistent with the facts, but that the facts are also consistent with the denial of spatial and temporal entities over and above things with spatial and temporal relations (Russell 1914a, p. 146):

> Hence, in accordance with Occam's razor, we shall do well to abstain from either assuming or denying points and instants.

Russell's answer to the metaphysicians was that he could state a tenable theory of particles, points, and instants, apparently thanks to Whitehead's idea of reconstructing such entities as classes of fundamental entities. Instead of the vague "intellectual constructions" of the psychologists, logical constructions could fulfil all the purposes of the hypothetical entities assumed by mathematical physics.

It may seem strange that Russell acknowledged his debt to Whitehead for the application of the method of definition by abstraction in the philosophy of physics, for he himself applied this method in the philosophy of mathematics as early as 1901. Apparently what was new for Russell in 1914 was the ontological purpose, though he came to see it differently afterwards. I shall return to this point in the next chapter; here I confine the discussion to Russell's use of classes outside the philosophy of mathematics.

In concluding this section, I turn to Russell's methodological observation that reconstructions are hypothetical. In Our knowledge of the external world, Russell considered first the problem of conceiving "momentary common-sense things" as classes of less hypothetical entities. The aim was the construction of a theory "which contains and places the experienced facts" of the world. Such a theory would demand hypotheses about the fundamental entities, reason for Russell to speak of a "hypothetical construction" (Russell 1914a, p. 93, p. 96, p. 97) or "a largely hypothetical picture of the world" (o.c., p. 93), in short "a possible theory" (o.c., p. 87). Let us see how Russell's view that reconstructions are hypothetical played a role in his treatment of common sense things.

An example of a "momentary common-sense thing" is a particular penny at a certain moment. Russell wanted to interpret the fact that this penny looks different in shape from different points of view. As a matter of fact, Russell transformed (R1) from <u>The problems of philosophy</u> into an hypothesis of his reconstruction:

(R1*) At each moment there is a different perspective from every different point of view.

The term 'perspective' refers to the fundamental entities of the reconstruction, (three-dimensional) visual views of the world: that each person who is not blind has such perspectives at each moment that he sees something, is an hypothesis, and so is the assumption that there are infinitely many such perspectives, even if there is no person who has them. Russell left no doubt about the hypothetical character of such assumptions when he applied (R1*) in a situation with more than one (!) person (Russell 1914a, p. 87-88):

> If two men are sitting in a room, two somewhat similar worlds are perceived by them; if a third man enters and sits between them, a third world, intermediate between the two previous worlds, begins to be perceived. It is true that we cannot reasonably suppose just this world to have existed before, because it is conditioned by the sense-organs, nerves, and brain of the newly arrived man; but we can reasonably suppose that <u>some</u> aspect of the universe existed from that point of view, though no one was perceiving it.

That there are indeed similarities between different perspectives, together with the assumption that a similarity can be greater or smaller, is another hypothesis. It enabled Russell to introduce a relation of neighbourhood between perspectives. He also assumed that between two perceived perspectives which are similar there is a series of other perspectives such that "between any two however similar, there are others still more similar" (Russell 1914a, p. 88):

> In this way the space which consists of relations between perspectives can be rendered continuous, and (if we choose) three-dimensional.

After having stated these hypotheses, Russell claimed to be able to define the momentary common-sense "thing" as opposed to its momentary appearances (Russell 1914a, p. 89):

(R2*) By the similarity of neighbouring perspectives, many objects in the one can be correlated with objects in the other, namely with the similar objects. Given an object in one perspective, form the system of all the objects correlated with it in all the perspectives; that system may be identified with the momentary common-sense "thing".

It is questionable whether a definition of this kind serves its purpose, but that is not what I want to discuss here. The important thing is that the example shows how "hypothetical" a reconstruction really is. A reconstruction does not consist of definitions alone, but is a theory of (part of) the material world, expressed in axioms about entities of a kind which are assumed to exist. Suppose that a certain reconstruction is "materially adequate" in the sense that a particular world-view is expressed in a rigorous theory, then the discussion can be opened whether the axioms of this theory are acceptable. However, one problem with the above sketch of a reconstruction of the common-sense view is that this view itself is a rather vague theory.

This makes it difficult to decide whether any reconstruction of the common-sense view can be considered materially adequate. Russell's "first rough sketch" does not make this easier. And then I am not even talking about the peculiar way in which he tried to reconstruct "the place where a thing is".

Similar objections can be raised against Russell's treatment of (more of less) permanent things, or physical objects. ("Things are those series of aspects which obey the laws of physics".) The whole approach is far removed from ontological reconstructionism in the style of Whitehead. Russell's sketch is no more a program than Mach's exposition was, though Mach could still allude to his physicalistic writings. (Cf. Mach 1919a, p. 297.) The criticism of one of the reviewers of <u>Our knowledge of the external world</u>, Theodore de Laguna, that Russell's constructions are theoretically inadequate as well as impossible in practice, would therefore have been fully justified, if Russell had not offered a reconstruction of the physico-mathematical time-series, where he could refer to the mathematico-logical treatment by Wiener. This reconstruction is the subject of the following section.

<u>A paradigm of reconstructionism: Russell's reconstruction of instants</u>

The physico-mathematical theory of time, as Russell called it, treats "time" as consisting of "instants" with certain properties. Russell stated what he considered these properties to be, while the possibility of a development of a rigorous theoretical system was shown by Norbert Wiener. Russell discussed this system.

I shall present a more detailed discussion of Russell's reconstructionism by explaining how in this particular case he solved various problems encountered in any formal ontological reconstruction. But before doing so, I shall reconsider Russell's starting point that mathematical instants are not things with which we are acquainted (Russell 1914a, p. 116):

> Even if there be a physical world such as the mathematical theory of motion supposes, impressions on our sense-organs produce sensations which are not merely and strictly instantaneous, and therefore the objects of sense of which we are immediately conscious are not strictly instantaneous. Instants, therefore, are not among the data of experience, and, if legitimate, must be either inferred or constructed. It is difficult to see how they can be validly inferred; thus we are left with the alternative that they must be constructed.

The problem, then, is the reconstruction of instants in terms of entities of the kind with which we are acquainted. In Our knowledge of the external world, the latter were posited almost without argument as "events" and their "temporal relations". But this does not mean that Russell was still as naïve about the givenness of time-relations as he had been in The problems of philosophy. There, he had claimed that one can perceive that a bell comes before another by "retaining" and then comparing them (1912a, p. 160):

> Suppose I hear a chime of bells: when the last bell of the chime sounds, I can retain the whole chime before my mind, and I can perceive that the earlier bells came before the later ones. Also in memory I perceive that what I am remembering came before the present time.

The reconstructionist Russell had to take psychological results seriously - in so far as they presented him with psychologically primitive data. Only a problem with psychological approaches, according to Russell, was that they "assumed, as a rule, a knowledge of physiology". Such knowledge, of course, is not psychologically primitive. This may explain why Russell did not give references to such approaches.

Regarding the experience of time however, Russell consulted James'

Psychology. He did this in the Monist-article "On the experience of time", where he went further than in the fourth lecture of Our knowledge of the external world. The question of how a relation of succession can be given was now answered as follows (Russell 1915a, p. 227):

> Succession may be immediately experienced between parts of one sense-datum, for example in the case of a swift movement; in this case, the two objects of which one is succeeded by the other are both parts of the present. It would seem that succession may also be immediately experienced between an object of immediate memory and a sense-datum, or between two objects of immediate memory. The extensions of our knowledge of succession by inference need not now concern us.

James, following E. R. Clay, used the notion of "specious present" in this connection, but Russell remarked that "this is a complicated notion, involving mathematical time as well as psychological presence": "The purely psychological notion which underlies it is the notion of one (momentary) total experience" (o.c., p. 219). The latter notion was explained in terms of a primitive notion of "being experienced together", a relation between objects of acquaintance.

The above relation of succession is quite important indeed, for it gave rise to the fundamental relations of Russell's reconstruction of instants and relations between instants (Russell 1915a, p. 227):

> We say that A is earlier than B if A is succeeded by B; and in the same case we say B is later than A. These are purely verbal definitions. It should be observed that earlier and later are relations given as between objects, and not in any way implying past and present. There is no logical reason why the relations of earlier and later should not subsist in a world wholly devoid of consciousness.

Thus, the relations of "earlier than" and "later than" are considered apart from the context of the experiencing subject. This involved an implicit change in terminology: members of the field of these relations were now called "events". As it appears, Russell's introduction of events in Our knowledge of the external world was rather elliptical (Russell 1914a, p. 116):

> Immediate experience provides us with two time-relations among events: they may be simultaneous, or one may be earlier and the other later.

So much for Russell's general point of departure. We are now in a position to state the different problems encountered in an ontological reconstruction satisfying requirements (1) and (2) of p. 90:

(I) the structure of the theory which one wants to reconstruct must be described explicitly,
(II) the question why this theory stands in need of reconstruction must be answered,
(III) why the ontology of the reconstructed theory is preferable to the ontology of the original theory must be explained,
(IV) one must take care that the reconstruction is materially adequate by giving suitable definitions and hypotheses,
(V) the acceptability of the hypotheses must be discussed.

Russell treated each of these problems in his reconstruction of instants; I therefore call it a paradigm of reconstructionism. The differences with that other paradigm of logical analysis, Russell's theory of descriptions, is conspicuous; there are no logical puzzles which have to be solved; instead, ontological problems are the heart of the matter.

(I):
Fortunately, for Russell there was no problem of "the correct interpretation" of the classical theory of time - as there might be in

the case of a common-sense view. He could formulate this theory in its current form: a dualistic theory, assuming two kinds of entities, instants and events, such that

(1) the set of instants is a one-dimensional continuum, ordered by a relation "earlier than",
(2) for each event there is precisely one (initial) instant such that the event is at this instant and not at any earlier one,
(3) if an event is at two instants, then it is at every instant "between" these instants. (An instant y is said to be between two instants x and z if x is earlier than y and y earlier than z, or z is earlier than y and y earlier than x.)

In Our knowledge of the external world, Russell restricted himself to the following "properties of instants":

(i) instants are linearly and densely ordered,
(ii) every event is at a first instant.

That Russell omitted the Dedekind-property (for short) of the set of instants can be explained in at least two ways. He could have thought that this "high degree of continuity" was not interesting for philosophers (cf. Russell 1914a, p. 132), or perhaps he had not yet found conditions for it in terms of the basic elements.

(II):

The main argument for a reconstruction of the above theory of time is that instants are not among the data of experience, whereas events, as Russell saw them, are. If the physical theory of time "is to consist wholly of propositions known to be true, or at least capable of being proved or disproved", instants must be either inferred or constructed from the data of experience. Since Russell found it "difficult to see how they can be validly inferred" - apparently on the ground of similar arguments as in the case of material objects - he was left with the alternative that instants must be constructed from events. (Cf. Russell 1914a, p. 111, p. 116.) When Russell considered events, he meant "events of which we are conscious" and such events "do not last merely for a mathematical instant, but always for some finite time, however short" (Russell 1914a, p. 116). If the classical theory has no axiom to the effect that events cannot last for just one instant, but recognizes "certain events as of an instantaneous character" (Robb 1913a, p. 9), then these events would also have to be reconstructed. But such a reconstruction would surpass Russell's primary aim of dealing with the question of time, restricted to "one private world". This excluded events such as two particles striking one another; a reconstruction of such instantaneous events presupposes a reconstruction of particles.

(III):

If the reconstructed theory is to be preferable to the original theory _from Russell's standpoint_, then it has to be shown that all the fundamental elements of the reconstructed theory are (possible) data of experience, that is, entities of the kind with which we are acquainted. However, Russell's defense of his ontology of events and time-relations between events in _Our knowledge of the external world_ was mainly concerned with a rejection of the Kantian view that "only the events are given, and their time-order is added by our subjective activity". The problem of how in a single act of consciousness one can be acquainted with a time-relation of "earlier than" between events was not posed. There is not even a reference to the earlier quoted passage that spatial and temporal relations, for example in the case of a swift motion wholly within the specious present, must be included among the facts of sense. But Russell did discuss such problems in a series of articles in _The Monist_, from "On the nature of acquaintance" to the above-mentioned "On the experience of time". I have already quoted from the last article in the introduction to this section. We saw that the relation of succession was the pivot on which everything hinged (Russell 1915a, p. 213):

> _Succession_ is a relation which may hold between two parts of one sensation, for instance between parts of a swift movement which is the object of one sensation; it may then, and

> perhaps also when one or both objects are objects of immediate memory, be immediately experienced, and extended by inference to cases where one or both of the terms of the relation are not present.
>
> When one event is succeeded by another, the first is called _earlier_ and the second _later_.

This is not the whole theory; especially important is Russell's characterization of "one sensation", or (more generally) "one experience" in terms of the relation of "being experienced together". He argued that this relation might be "best taken" as a simple or ultimate relation among objects which itself is "sometimes immediately experienced as holding between two objects", though it might also hold when it is not perceived (Russell, o.c., p. 216). A problem with this relation is that it is not transitive, so that the way of defining "one experience" as an equivalence class was not open. But Russell solved this problem conveniently (Russell, o.c., p. 217-218):

> We can, however, avoid all difficulties by defining "one (momentary) total experience" as a group of objects such that _any two_ are experienced together, and nothing outside the group is experienced together with all of them.

The definition itself is already interesting; it shows how one can form classes with the help of a relation which is reflexive and symmetrical, but not transitive, a fact that is not without significance for the subsequent reconstruction of instants.

(IV):
In _Our knowledge of the external world_, Russell did not prove that his reconstruction of instants was materially adequate. This fell outside the scope of his book (cf. Russell 1914a, p. 8), which may explain why his presentation of the system is partially defective. However, Russell is excused since he referred to Wiener's mathematico-logical treatment.

The theory is roughly as follows. There are fundamental time-relations among events: "they may be simultaneous, or one may be earlier and the other later". A relation of "wholly preceding" is defined as follows: "when one event is earlier than, but not simultaneous with another, we shall say that it 'wholly precedes' the other". An "instant" is defined as any group of events with the property that any two are simultaneous with each other, and no event outside the group is simultaneous with all of them. In other words, instants are reconstructed as maximal groups of mutually simultaneous events. An event is said to be "at" a (defined) instant when it is a member of that instant. An instant is considered to be "before" another instant when there is an event at the first instant, wholly preceding an event at the second instant. An auxiliary definition calls an event an "initial contemporary" of an event when it is (1) simultaneous with (or a "contemporary" of), and (2) not wholly after anything simultaneous with this event.

Wiener showed that instants defined as above are linearly ordered by the relation "before", if the following assumptions are made [35]:

- No event wholly precedes itself,
- if one event wholly precedes a second event simultaneous with a third, and this third wholly precedes a fourth, then the first wholly precedes the fourth,
- if two events are simultaneous, then neither of them wholly precedes the other.

Russell found that an event is at a first instant if

- every event wholly after some contemporary of a given event is wholly after some initial contemporary of it.

Finally, the series of instants will be densely ordered - "compact" in Russell's terminology - if the following assumption is made (Russell 1914a, p. 120):

- If one event wholly precedes another, there is an event wholly

> after the one and simultaneous with something wholly before the other.

It can scarcely be overestimated how important it is that Russell took care of the problem of the adequacy of his reconstruction, though for the mathematico-logical treatment of the proofs he referred to Wiener's paper "A contribution to the theory of relative position". After giving his definition of "an instant of time", he listed a number of properties which the instants of the classical theory of time are supposed to have. Finally, he enumerated assumptions which guarantee that defined instants also have these properties.

(V):

We have seen that ontological reconstructions are hypothetical in the sense that hypotheses about the fundamental elements are assumed. It is clear that the acceptability of such a reconstruction stands or falls with the acceptibility of its assumptions. Therefore each ontological reconstruction demands a discussion how far these assumptions can be justified.

Russell provided such a discussion after his sketch of a reconstruction of a "thing", which he considered "a largely hypothetical picture of the world, which contains and places the experienced facts, including those derived from testimony". Russell especially examined the hypothesis "that other people have minds", and concluded that it was mainly supported by an argument from analogy which a mystic does not need to accept. But, he added, "it is a hypothesis which systematizes a vast body of facts and never leads to consequences which there is reason to think false".

The hypothetical character of the reconstruction of instants, becomes manifest, on the last assumption (Russell 1914a, p. 120):

> Finally, the series of instants will be compact if, given any of two events of which one wholly precedes the other, there

> are events wholly after the one and simultaneous with something wholly before the other. Whether this is the case or not is an empirical question; but if it is not, there is no reason to expect the time-series to be compact.

In a footnote with a list of the assumptions "made concerning time-relations in one experience", the last assumption is again examined in a similar way (Russell 1914a, p. 120):

> This assumption entails the consequence that if one event covers the whole of a stretch of time immediately preceding another event, then it must have at least one instant in common with the other event; i.e. it is impossible for one event to cease just before another begins. I do not know whether this should be regarded as inadmissible.

The first quotation seems to indicate that Russell was prepared to reject part of the classical theory - at least as it was applied to psychology - in the case that one of the hypotheses of a reconstruction would turn out to be untenable. The conclusion that there was as yet no empirical base for a compact time-series would have been less dogmatic. In the fifth lecture of Our knowledge of the external world, Russell did discuss the general question "Is there, in actual empirical fact, any sufficient reason to believe the world of sense continuous?". The answer was negative, for familiar reasons (o.c., p. 147-148). There remained (only) the advantage that the hypothesis of continuity is "technically simpler than any other hypothesis". However, a closer inspection of the last hypothesis brought out new aspects. The problem is that the assumption in question "requires that the number of events should be infinite in any finite period of time". Imagine what this amounts to (Russell 1914a, p. 149):

> If this is to be the case in the world of one man's sense-data, and if each sense-datum is to have not less than a certain finite temporal extension, it will be necessary to

> assume that we always have an infinite number of sense-data simultaneous with any given sense-datum.

Russell reacted somewhat differently on this point in the two editions of Our knowledge of the external world. The following summary shows how seriously Russell took the formal ontological reconstructionist philosopher's task of defending or justifying his assumptions.

In the first edition, he saw only two alternatives, "either declare that the world of one man's sense-data is not continuous, or else refuse to admit that there is any lower limit to the duration (...) of a single sense-datum". Obviously, we no longer have a reconstruction of the physico-mathematical time-series with the first alternative, while the second contravened his earlier statements. However, he did not here commit himself to either of these alternatives: "I do not know what is the right course to adopt as regards these alternatives" (Russell 1914a, p. 149-150). He called the empirical decision between the various hypotheses "a problem for the psychologist".

In the second edition, Russell repeated his conviction that experienced events have "a duration which cannot sink below a certain minimum". Now he saw three ways of rescuing the reconstruction: (1) bringing in events wholly outside our experience, (2) assuming that experienced events have parts which we do not experience, or (3) postulating that we can experience an infinite number of events at once (Russell 1926a, p. 126). He also added a few lines to the text of lecture 5, leaving out the above quotation, and committed himself in so many words to the second alternative, acknowledging that sense-data have parts which are not sense-data (1926a, p. 156). Thus, the ultimate existents of the reconstruction would comprise not only perceived or unperceived but perceptible events, but also imperceptible events. This requires a defense of the assumption of imperceptible events independent of the argument that the assumption enables us to reconstruct the physico-mathematical time-series. Russell was not a shallow philosopher: he produced just such a defense in The analysis of matter (Russell 1927a,

p. 280-283) [34].

This ends my exposition of Russell's reconstruction of instants. Technical details can be found in Wiener's paper and in "On order in time". It is not neccesary to consult these articles to understand what follows about Russell's formal apparatus.

Considerations on the use of the calculus of classes in formal ontological reconstructions

The formal apparatus of Russell's reconstruction of instants was the calculus of classes and relations from the sections C, D, and E of Part I ("Mathematical logic") of the first volume of Principia mathematica. The main point was that one can reconstruct (1) entities, assumed by a theory, as classes of members of the field of given relations, and (2) relations between these entities as relations between such classes.

Russell realized that the recourse to classes might surprise an unprepared audience (Russell 1914a, p. 124):

> When a point or an instant is defined as a class of sensible qualities, the first impression produced is likely to be one of wild and wilful paradox.

Postponing "certain considerations" to a later lecture, he then tried to remove such first impressions by explaining the method of definition by abstraction in which equivalence classes take the role of "common properties". After having mentioned some examples of (non-null) transitive symmetrical relations, he gave a defense of the use of this method, echoing the justification given in section 111 of The principles of mathematics (cf. section 1 of Chapter Four) (Russell 1914a, p. 125-126):

> In all such cases, the class of terms that have the given transitive symmetrical relation to a given term will fulfil

> all the formal requisites of a common property of all the members of the class. Since there certainly is the class, while any other common property may be illusory, it is prudent, in order to avoid needless assumptions, to substitute the class for the common property which would be ordinarily assumed.

This method was said to be the only safe one, because it avoided "the risk of introducing fictitious metaphysical entities" for cases in which there are no such "common properties". But how could Russell claim that "there certainly is the class" when suitable conditions are satisfied? My answer to this question is that Russell only meant to say that when certain entities are given, together with hypothetical analogues of them, we can form classes of these entities. Such classes are "logical functions" of the given entities, or "constructions" out of these entities. They "are there" so to speak, as soon as they are made. In this sense Russell's claim can be understood.

But now for the "certain considerations" which apply here. Russell treated them after the reconstruction of numbers as classes of classes, in lecture 7 of Our knowledge of the external world. The problem is stated as follows (Russell 1914a, p. 205):

> There is, however, a certain logical doctrine which may be thought to form an objection to the above definition of numbers as classes of classes - I mean the doctrine that there are no such objects as classes at all. It might be thought that this doctrine would make havoc of a theory which reduces numbers to classes, and of the many other theories in which we have made use of classes.

Russell believed that it did not (o.c., p. 205-206):

> This, however, would be a mistake: none of these theories are any the worse for the doctrine that classes are fictions.

This is followed by a brief exposition of the doctrine, with the purpose, among others, of explaining why it is not destructive. The outcome was a special view on logic and mathematics: their apparent objects are not actual entities, they are only concerned with logical forms. Ontological considerations are out of place here, and this seems enough for drawing a sharp boundary between logical analysis and ontological reconstructionism. Let us see how this position was taken up.

There are two main problems connected with the doctrine that "there are no classes":

(I) If logical theory implies that there are no classes, how can we appeal to them in ontological reconstructionism, as we do when making instants classes of events?

(II) If logical theory makes classes symbolic constructions, as it does, why is this not an example of an ontological reconstruction?

The answer to the latter question is contained in the view that the so-called logical constants are not entities in an ontological sense. But let us first see what the doctrine that there are no classes roughly amounts to.

Russell's theory of classes gives a "uniform method of interpreting propositions in which a <u>verbal</u> use of classes occurs, so as to obtain propositions in which there is no longer any such use" (cf. Russell 1914a, p. 207): "all statements nominally about a class can be reduced to statements about what follows from the hypothesis of anything's having the defining property of the class". According to this theory, a proposition which grammatically is about a class is to be regarded as really concerned with a propositional function and an apparent variable whose values are so-called <u>predicative</u> propositional functions (cf. Whitehead and Russell 1910a, summary of Part I). With this logical theory, classes no longer "count as one" in the sense that they are not "entities" from a logical point of view. The Chinese philosopher Hui Tzu, who maintained that "a bay horse and a dun cow are three; because taken separately they are two, and taken together they are one: two and one make three" (Russell 1914a, p. 206), was wrong from a logical point of view: by a statement nominally about a class, such as "Mankind is fond of apples", we do not mean that there is one individual, called "mankind", who munches apples (Russell 1914a, p. 206). What we mean by such a statement is given in the interpretation indicated above, in which there is no longer talk about "classes". The doctrine even applies to so-called combinatorial problems (Whitehead and Russell 1910a (1927a, p. 74)):

> And if we take such simple problems as "how many combinations can be made of n things?" it seems at first sight necessary that each "combination" should be a single object which can be counted as one. This, however, is certainly not neccessary technically, and we see no reason to suppose that it is true philosophically.

As soon as classes do not "count as one", they are no longer "entities" in a logical sense. This is roughly what the doctrine that there are no classes amounts to.

It was asked how classes can be retained in ontological reconstructionism, when "there are no classes" in the sketched sense. The answer to this question is given with Russell's introduction of class symbols in such a way that their use corresponded in general to the use of classes in ordinary thought and speech (cf. Whitehead and Russell 1910a, (1927a, p. 24)), without producing any conflict within logical theory. He achieved this by the so-called doctrine of incomplete symbols in which the <u>uses</u> of in themselves eliminable <u>class symbols</u> are defined, so that <u>classes</u> became "merely symbolic or linguistic conveniences, not genuine objects as their members are if they are individuals" (o.c., p. 71-72).

We can therefore continue speaking of, say, instants as classes of events, without committing ourselves to the view that they are entities from a logical point of view. Nor are they entities in the relevant ontological sense. In a reconstruction of instants, the only entities in this sense are events - "ultimate existents" as Whitehead called them - and relations among events.

The theory which defines the notion of "class" in terms of (other) logical notions, is so similar to the theory which defines instants in terms of events, that one might consider the first theory just an ontological reconstruction of the second. This applies especially to the reconstruction of numbers as classes of classes; here ordinary propositions about numbers are analyzed as statements about classes of classes. Such statements are to be interpreted as propositions in which no use is made of the notion of class, but only expressions for (other) logical notions occur. Are these logical notions in an ontological sense the fundamental entities of logic and mathematics? Russell answered this question in the negative in the last paragraph of lecture 7 of Our knowledge of the external world (Russell 1914a, p. 208):

> If the theory that classes are merely symbolic is accepted, it follows that numbers are not actual entities, but that propositions in which numbers verbally occur have not really any constituents corresponding to numbers, but only a certain logical form which is not a part of propositions having this form. This is in fact the case with all the apparent objects of logic and mathematics. Such words as or, not, if, there is, identity, greater, plus, nothing, everything, function, and so on, are not names of definite objects, like "John" or "Jones", but are words which require a context in order to have meaning. All of them are formal, that is to say, their occurrence indicates a certain form of proposition, not a certain constituent. "Logical constants", in short, are not entities; the words expressing them are not names, and cannot

> significantly be made into logical subjects except when it is the words themselves, as opposed to their meanings, that are being discussed. This fact has a very important bearing on all logic and philosophy, since it shows how they differ from the special sciences. But the questions raised are so large and so difficult that it is impossible to pursue them further on this occasion.

That Russell had earlier thought otherwise of logical constants can be seen by the standpoint he took in "L'importance philosophique de la logistique" or "The philosophical implications of mathematical logic".

How Russell ontologized logical theory will occupy us in the next Part.

RUSSELL AND WITTGENSTEIN

PART THREE

RUSSELL AND WITTGENSTEIN

Introduction to Part Three

Throughout the history of philosophy, philosophers have tried to defend philosophical positions with the help of argumentation in natural language. Sometimes so-called paradoxes played a role; think of Zeno's paradoxes of movement and Kant's antinomies. According to modern analytic philosophers, such paradoxes and the accompanying argumentation lend themselves pre-eminently to logical analysis. Already in his book The principles of mathematics, Russell showed how logical and mathematical analysis could indeed contribute to a better understanding of the above-mentioned examples of paradoxes.

An easy objection to the application of logical and mathematical theories to philosophical problems is that such theories themselves rest on philosophical presuppositions. In Russell's case, his so-called pluralism would be an example of such a philosophical, and in particular ontological assumption. I do not agree with this diagnosis, simply because the ordinary or philosophical notion of existence has no place in Russell's early theory of logic and mathematics. (It is true that Russell discussed ontological questions, but he did so outside the context of theory of logic, as in the case of his examination of different theories of time.)

My view is supported by Russell's own explanation in "The existential import of propositions". Here he distinguished two meanings of the word 'existence', (a) a philosophical or common-sensical, (b) a technical sense, occurring only in logic and mathematics. The first meaning does not occur in logic and mathematics: "All entities, whether they exist or whether they do not (in sense (a)), are alike real to symbolic logic and mathematics" (Russell 1905a, 1973a; p. 99).

For Russell, logic and mathematics are not concerned with philosophical questions of existence: "The number 2, or the principle of the syllogism, or multiplication are objects which mathematics considers, but which certainly form no part of the world of existent things" (Russell 1905a; 1973a, p. 98). When Russell claims that these entities have "being", he does not, in my view, embrace a kind of realism - at least in the context of the logical analysis of mathematics given in The principles of mathematics (cf. Chapter Four). Being, belonging to "whatever can be counted", was a logical category. And indeed, we do count numbers, rules of deduction, and operations. (One does not have to be a Fregean to see that.) However, we find that Russell later expresses the view that "logic is concerned with the real world just as truly as zoology, though with its more abstract and general features" (Russell 1919a, p. 169). Though this quotation must be placed in its right context, it seems that we here no longer have two completely different and separated fields of research, logic and mathematics on the one hand, and the study of the real world on the other hand. As a matter of fact, the individual variables of the logical theory of Principia mathematica were seen to range over actual particulars. Russell had become engaged in a kind of philosophical logic, in which both logical and ontological questions were at home.

Wittgenstein came close to this position when he said that "logical propositions describe the scaffolding of the world" - Die logischen Sätze beschreiben das Gerüst der Welt, oder vielmehr sie stellen es dar - (Wittgenstein 1921a, p. 253, 6.124). The elementary propositions which the logical theory of his "Logisch-Philosophische Abhandlung" presupposed were seen to describe (possible) facts in the world.

The first chapter of Part Three is concerned with the development of Russell's thought preceding both his and Wittgenstein's hybrid doctrines of "philosophical logic". I argue that the study of Meinong gave Russell a new orientation which eventually brought extra-logical considerations into logical theory. Wittgenstein saw this and tried to let "logic take care of itself", but the outcome of his investigations was that logic is subject to the (mystical) presupposition "that the world is". (Cf. Wittgenstein 1921a, p. 261, 6.44.) Eventually, both philosophers gave logical theory an extra-logical turn by employing the idea of a language in which the world can be described completely. This resulted in a confusion of logical analysis with ontological reconstruction. How this could take place is shown in the two following chapters of Part Three in which Wittgenstein's and Russell's doctrines of "logical philosophy" are discussed.

The rest of Part Three is devoted to a discussion of the question whether the intrusion of ontology into logical theory is inevitable. After discussing Ramsey's philosophy of logic, I shall indicate in the Epilogue how later developments in philosophical logic might be evaluated.

CHAPTER SEVEN

THE BEGINNINGS OF "PHILOSOPHICAL LOGIC"

After Russell had finished The principles of mathematics he set himself at least two tasks: the elimination of the contradiction in the theory of classes within the project of the logical analysis of arithmetic, and the resolution of the unanswered questions on the subject of denoting. A third task was soon added: to develop a position on Meinong's theory of objectives or, more generally, the formulation of a satisfactory explication of the notions of truth and falsehood. Eventually, the results of Russell's investigations were reported in the first volume of Principia mathematica. Though this work is explicitly devoted to a logical analysis of mathematics, it includes extra-logical considerations, notably Russell's explication of the notions of truth and falsehood. This was done in such a way that a reader with a philosophical interest might think that these considerations are indispensable for the logical theory in question. Russell himself encouraged this view by writing a paper on "the philosophical implications of mathematical logic". This article, together with his book The problems of philosophy, contains several starting-points for a reconsideration of "the foundations of logic".

In this chapter, I sketch aspects of Russell's work on the above-mentioned tasks which may be seen as having paved the way for the later intrusion of ontological discussions into logical theory. I deal successively with Russell's critique of Meinong, the new theory of denoting, problems of propositions and classes, and Russell's early philosophical interpretation of the logical theory of Principia mathematica.

Russell's critique of Meinong

The early influence of Meinong in the English-speaking world is a remarkable phenomenon in the history of philosophy. Why was Meinong almost immediately read and Husserl not? It has been said that Meinong's influence was due to the interest taken in his writings by Russell. (Cf. Findlay 1952a, p. 12.) But why did Russell show interest in Meinong? Was it by accident that he came across Meinong's publications around 1900? We know that Russell was very impressed by Meinong's paper "Ueber die Bedeutung des Weberschen Gesetzes", so it seems plausible to assume that he was anxious to see more work of "so excellent a writer" (Russell 1903a, par. 405).

It can already be seen from the first paragraphs of "Meinong's theory of complexes and assumptions" that Russell was not disappointed when he read Meinong's publications "Ueber Gegenstände höherer Ordnung und deren Verhältnis zur inneren Wahrnehmung" and Ueber Annahmen; he welcomed Meinong's standpoint in theory of knowledge since he saw it as coming close to his own views. It was a good opportunity to think about such problems. That there are problems in this field of research becomes clear as soon as it is realized, as Russell did in The philosophy of Leibniz, that the inquiry into the nature of knowledge is "hybrid, and subsequent both to the philosophical discussion of truth, and to the psychological discussion of belief", for "in discussing knowledge, i.e. the belief in a true proposition, we presuppose both truth and belief" (Russell 1900a, par. 98).

In "Meinong's theory of complexes and assumptions", Russell again turned to the question of a delineation of the subject-matter of theory of knowledge. His view had not changed much (Russell 1904a; Russell 1973a, p. 22):

> The theory of knowledge is in fact distinct from psychology, but it is more complex: for it involves not only what psychology has to say about belief, but also the distinction of truth and falsehood, since knowledge is only belief in what is true. Thus the subject may be approached either through psychology or through logic, both of which are simpler than it is.

Theory of knowledge, then, presuposses both theory of logic and psychology, in so far as they are concerned with truth and falsehood and with belief respectively. Does this mean that logic and psychology are linked in theory of knowledge? This depends on whether the objects of belief are identified with the vehicles of truth and falsehood. We shall see that this was done by Russell, though he didn't go so far as to admit extra-logical arguments in his theory of logic.

That two different disciplines are involved in epistemology may lead to different approaches in theory of knowledge. It is obvious that, since Meinong approached it through psychology - "but with great logical acumen" (Russell 1904a; 1973a, p. 22) - Russell found it interesting "to confront his views with views which are suggested by the approach through logic" (o.c.). His treatment of Meinong's doctrine of the objects of a judgment, called objectives (<u>Objectiven</u>) is a conspicuous example of this approach. Russell seems to have interpreted this doctrine as a logical theory by identifying Meinong's notions of "objective" and "subsistence" (<u>Bestand</u>, <u>Sein</u>) with his own logical notions of "proposition" and "being". This may have been what prompted Russell to conclude that Meinong did not attribute <u>being</u> to <u>false propositions</u> when he considered the objects of false judgments "non-subsisting objectives", in contrast to Russell's own view that "logic must concern itself as much with false propositions as with true ones" (Russell 1904a; 1973a; p. 58). In Russell's view, Meinong fell pray to a kind of psychologism when he considered false propositions "the non-subsisting, merely pseudo-existing objectives of erroneous judgements". For, as soon as false propositions figured in Meinong's alleged logic, it becomes necessary to take account of judgment and this makes psychology, in a sense, more fundamental than logic (cf. Russell 1904; Russell 1973, p. 58). Russell rejected this. For him, the acceptability of (meta-)logical distinctions did not depend on psychological distinctions. His defense of the "transcendence" of false propositions employs logical, not psychological arguments. It makes use of the argument that when we say 'Your going to town was most adventurous' or 'Your going to town would have been most unwise', the adventurousness and lack of wisdom do not apply to a judgment but to a proposition, whether true of false (cf. Russell 1904a; Russell 1973a, p. 73). Thus Russell arrived at the position "that there are, apart from and indepently of judgment, true and false propositions, and that either kind may be assumed, believed of disbelieved".

We find the last claim in the following well-known statement of Russell's position (Russell 1904a; Russell 1973a, p. 75):

> It may be said - and this is, I believe, the correct view - that there is no problem at all in truth and falsehood; that some propositions are true and some false, just as some roses are red and some white; that belief is a certain attitude towards propositions, which is called knowledge when they are true, error when they are false.

Lackey, in his introduction to the edition of Russell's papers on Meinong, concluded that propositions "serve" as the objects of mental states (Russell 1973a, p. 18). If Lackey is right, then we have here an example of a realistic interpretation of a logical notion. It provides Russell with a possibility of using extra-logical arguments with respect to logical theory. Such arguments indeed seem to be involved in the penultimate paragraph of "Meinong's theory of complexes and assumptions". But Russell realized that they were not logical. The paragraph starts with a summary of what Meinong might have had in mind when he rejected false objectives, namely: true propositions express "fact", while false ones do not. Russell reduced Meinong's rejection of false objectives to the argument that "when a proposition is false, something does not subsist which would subsist if the proposition were true". He concluded that the resulting theory does not regard affirmative propositions such as "A exists" and negative propositions, such as "A does not exist" on the same level:

> The point involved, therefore, comes to this; it is hard to

> regard A's non-existence, when true, as a <u>fact</u> in quite the same sense in which A's existence would be fact if it were true.

Russell doesn't accept this argument because it is not a logical one (Russell 1904a; Russell 1973a, p. 76):

> It may be suspected, however, that this apparent difference is not logical, but derived from the nature of perception: all the propositions we perceive are affirmative, and the word <u>fact</u> applies most naturally to propositions which are either perceived or analogues to such as are perceived. It would seem that all the negative propositions which we believe are derived by inference from affirmative propositions, by means of implications of the form 'p implies not-q'; and this seems sufficient to account for the feeling that true affirmative propositions express fact in a sense in which no others do so.

Obviously, Russell was aware of a distinction between logical and non-logical differences, although he gave no criterion for it [36]. Therefore I have some reservations about saying that Russell wanted to treat Meinong's doctrine solely as a logical theory. (In that case his identification of Meinong's objectives with propositions would be justified.) Nevertheless, he had a more or less implicit idea of what a logical argument was, witness the following (Russell 1904a; Russell 1973a, p. 61):

> If I believe that A is the father of B, I believe something; the subsistence of this something, if not directly obvious, seems to follow from the fact that, if it did not subsist, I should be believing nothing, and therefore not believing. And it is plain that others may believe the <u>same</u> thing; this, however, might be regarded as implying only sameness of content. Again, it is possible to count propositions, to make

> classes of them, and so on; but in doing so it is by no means necessary to confine ourselves to true propositions.

From Russell's point of view, it is indeed very difficult, if not impossible, to accept "non-subsisting objects" in the sense of objects without being. For this would imply that "there are objects for which it is the case that they are not". As we know, Meinong also found this (at least) paradoxical and tried to escape the paradox primarily by a recourse to "certain psychological experiences" - <u>gewisse psychische Erlebnisse</u> - by letting non-subsisting objects "exist-in-the-imagination" and claiming that they have "pseudo-existence". Later he was not satisfied with this solution, as he admitted in his paper "Ueber Gegenstandstheorie". So he took the famous position that the whole opposition of subsistence and non-subsistence is only a question of objectives, not of objects. This means, for example, that in any case one of the objectives "the round square subsists" and "the round sqaure does not subsist" subsists. (Cf. Meinong 1904a, p. 13: "der Gegenstand ist von Natur ausserseiend, obwohl von seinen beiden Seinsobjektiven, seinem Sein und seinem Nichtsein, jedenfalls eines besteht".) This already seems sufficient for showing that Meinong's notion of subsistence (<u>Sein</u>) does not coincide with Russell's notion of being, so that Russell could not represent Meinong's doctrine as a kind of logical theory comparable with his own theory without distorting it. For though the round square is an object, it does not subsist in Meinong's view. And this was not even the end of the matter; Meinong attributed to such objects a so-called "so-being" - <u>Sosein</u> - which was independent of being - <u>Sein</u> - . According to Meinong, this explains why we can truly judge that the well-known golden mountain is golden, and the round square is round, as well as quadrilateral (and therefore also not round).

We can imagine Russell's reaction when he read Meinong's "Ueber Gegenstandstheorie". If, in analogy with his theory about false propositions, he would attribute "being" to the above-mentioned round square, then he had to face the problem that the propositions "the round square is round" and "the round square is not round" cannot both be true. The best way out, therefore, seemed to be to drop the assumption that the round square is an object, or has being, at all. But then the problem had to be solved how a proposition such as "the round square is round" is composed if it is not concerned with an object the round square. An answer was given with Russell's new theory of denoting.

Russell's search for a new theory of denoting

Though, technically speaking, Russell's new theory of denoting of 1905 is common-place, it is certainly not easy to capture its tenor in a non-anachronistic way. Wittgenstein's comment that Russell showed that the apparent logical form of a sentence need not be its real form (Wittgenstein 1921a, p. 212, 4.004) does not precisely describe Russell's contrast between "a wrong analysis of propositions whose verbal expressions contain denoting phrases" and the "proper analysis".

Already in The principles of mathematics, for example in the chapter on implication and formal implication, Russell made clear that propositions have to be "analyzed". In his view, what we would now call quantification theory "requires a thorough analysis of the constituents of propositions" (Russell 1903a, par. 45). In the subsequent discussion he concluded, for example, that the proposition "I met a man" although not being about the concept "a man", but in some sense, about an actual man, still contains this concept. He therefore invented a special theory of denoting which in this case would say that the concept "a man" denotes a so-called variable disjunction: if the (finitely many) members of the class of all men are a_1, a_2, a_3, ..., a_n, then "a man" denotes a_1 or a_2 or a_3 or ... a_n, "where or has the meaning that no one in particular must be taken" (Russell 1903a, p. 61). Similar treatments were given to the denoting of other denoting concepts, such as "all men", "every man" and "some man". This theory requires, for each such concept, an object, characterized as "a set of terms combined in a certain way, which something is denoted by all men, every man, any man, a man or some man; and it is with this very paradoxical object that propositions are concerned in which the corresponding concept is used as denoting" (Russell 1903a, par. 62). Perhaps this explains why Russell interpreted Meinong's theory of denoting as comparable with his own. For Meinong's theory also requires "paradoxical objects". Only in the case of denoting concepts of the form "the so-and-so", Russell was not very clear as to what such concepts denote. In a comment on the proposition "Edward VII is the king" he said that "Edwards form a class, and that seventh Edwards form a class having only one term": "Edward VII is practically, though not formally, a proper name". Does this imply that a denoting concept always denotes a class? Russell also claimed that when denoting concepts are introduced in an assertion of an identity, "there is involved, though not asserted, a relation of the denoting concept to the term, or of the two denoting concepts to each other". Does this imply that we should analyze the proposition "the present Pope is the last survivor of his generation" as a relation between two concepts? If so, what kind of relation is this, if "the is which occurs in such propositions does not itself state this further relation, but states pure identity"? "Involving", "asserting", "stating" - how do we unravel these notions? Russell was aware that this theory of denoting contained difficulties. In par. 75 of The principles of mathematics, he wrote that there were puzzles in this subject which he did not yet know how to solve. Fortunately, his new theory of denoting freed him from such difficulties.

The new theory was explicitly presented as a correct analysis of propositions, and, accordingly, as a logical theory. I conclude that it was not intended as a contribution to a Meinongian Gegenstandstheorie, let alone to ontology. Russell once remarked that the concept "a man" "does not walk the streets, but lives in the shadowy limbo of the logic-books" (Russell 1903a, par. 56). In "On denoting" this concept disappeared completely, together with all other denoting concepts of The principles of mathematics.

In "On denoting", the theories of Meinong and Frege were (also) treated solely as logical theories. Russell rejected the first theory because of problems with the law of contradiction, the second both because of its artificiality in treating meaningful denoting phrases without corresponding denotation and because of "certain rather curious difficulties", which will be discussed presently. His new theory, on the other hand, was "tested by its capacity for dealing with puzzles", all logical in character.

Russell discussed three such puzzles: the problem of George IV who wished to know whether Scott was the author of Waverley whereas he was not so much interested in the question whether Scott was Scott; the problem of the truth or falsity of the two statements 'the present King of France is bald' and 'the present king of France is not bald'; and finally, the problem of how a "non-entity can be the subject of a proposition", for instance in the case of the truth of the statement 'the difference between A and B does not exist' (Russell 1905b, p. 485).

The theory was also said to be important for the theory of knowledge; and this creates the problem whether the resulting application of the new theory of denoting had repercussions for the status of that logical theory. Another problem concerns Russell's alleged refutation of Frege's distinction of <u>Sinn</u> and <u>Bedeutung</u>. The logical character of his arguments can be questioned. The next section is devoted to this subject. It is followed by a section on theory of denoting and theory of knowledge.

<u>The first line of Gray's Elegy</u>

Russell's attack on the distinction between the meaning of a denoting phrase and its denotation is notorious for its lack of clarity. Hochberg (1976a), who listed a number of negative commentaries on Russell's discussion, tried to rehabilitate Russell by defending at least five claims, to wit: (1) Russell understood Frege, (2) Russell also understood how his earlier view was related to Frege's view, (3) Russell's arguments were directed against Frege, (4) Russell's statement of Frege's view is correct, and (5) Russell's arguments are cogent. Hochberg emphasized that Russell wanted to give an account of the relation of meaning and denotation that was not "merely linguistic through the phrase". Eventually he concluded that Russell was looking for "a philosophical, or metaphysical, or ontological analysis" in the sense of a specification of the entities, and relations among them, which were needed "to account for the fact being analyzed" (Hochberg 1978a, p. 175). If this was indeed Russell's purpose, then we have here an early example of a contamination of logical analysis with ontological reconstruction. However, I shall argue that Russell's discussion does not need ontological arguments to be understandable. It is possible, in my view, to get rid of the difficulties which Russell encountered when he tried to find out how both Frege - in "Ueber Sinn und Bedeutung" - and he himself - in The principles of mathematics - could coherently say that "the meaning denotes the denotation". The outcome of my exposition enables me to take a stand on Hochberg's claims.

Russell's argument that the distinction between "meaning" and "denotation" is wrongly conceived can be summarized as follows:

- The theory of meaning and denotation is based on the following principle: whenever we want to speak about an object, we must use an expression with a certain meaning such that this meaning denotes the object.
- If a certain meaning denotes a certain object, we can characterize this object as the denotation of that meaning.
- There are two ways of speaking in concreto about the denotation of a certain meaning:

  (1) we speak about the denotation of the meaning of, say, the expression 'the first line of Gray's Elegy'. This way of speaking, though unproblematic, is not very informative; the denotation of the meaning of the expression 'the first line of Gray's Elegy' is the first line of Gray's Elegy;

(ii) we speak about the denotation of ..., where the dots are replaced by an expression which denotes the meaning of the expression 'the first line of Gray's Elegy' without mentioning the expression 'the first line of Gray's Elegy' itself. However, if we try to do this with an expression in which the expression 'the first line of Gray's Elegy' itself is not mentioned but <u>used</u>, then such an expression will be concerned with 'The curfew tolls the knell of parting day', and the connection with the wanted meaning is broken.

Intuitively we would like to introduce double quotes and say that "the first line of Gray's Elegy" is the wanted meaning. This can be done. We must realize, however, that we then have to repeat the analysis for '"the first line of Gray's Elegy"'. (We can speak about this line in several ways!) According to the theory of meaning and denotation, <u>this</u> last expression should also have or express a meaning. But what is this meaning? Either it is the <u>denoted</u> meaning, in which case meaning and denotation coincide, or it is something else. But what? Since there is no answer to this question, one cannot but conclude that the theory of meaning and denotation is ill-conceived.

Let us see what is right and what is wrong with this argument, by taking a closer look at Russell's line of thought.

According to Russell, "when we wish to speak about the <u>meaning</u> of a denoting phrase, as opposed to its <u>denotation</u>, the natural mode of doing so is by inverted commas". Examples are "the centre of mass of the Solar System", which Russell called a denoting complex, as opposed to the centre of mass of the Solar System itself, which is a point, and "The first line of Gray's Elegy" and the first line of Gray's Elegy (Russell 1905b, p. 480):

> Thus taking any denoting phrase, say C [for example 'the first line of Gray's Elegy' - H.V.], we wish to consider the

> relation between C [here: the first line of Gray's Elegy - H.V.] and "C" [here: "the first line of Gray's Elegy" - H.V.], where the difference of the two is of the kind exemplified in the above two instances.

The difficulty with this quotation is that Russell used the letter 'C' both for indicating a denoting phrase and for speaking about its denotation. The next statement is completely understandable (o.c.):

> We say, to begin with, that when C [take any denoting phrase - H.V.] occurs it is the denotation that we are speaking about [when we are using the denoting phrase in question - H.V.]; but when "C" [the denoting phrase in double quotes - H.V.] occurs, it is the meaning.

This is followed by the crucial question about meaning and denotation (o.c.):

> Now the relation of meaning and denoting is not merely linguistic through the phrase: there must be a logical relation involved, which we express by saying that the meaning denotes the denotation.

Apparently, Russell was not satisfied with saying that a denoting phrase has two sides, a meaning and a denotation - these being related because they are "assigned to" the same phrase. (This would only give a "merely linguistic relation through the phrase".) Earlier in the article, Russell stated that in this theory we "shall say that the denoting phrase expresses a meaning; and we shall say both of the phrase and of the meaning that they denote a denotation". The first two formulations are translations from Frege ("Ueber Sinn und Bedeutung", p. 31) who also said that the meaning (of a symbol) "corresponds" with a certain denotation (Frege 1892a, p. 27):

> Die regelmässige Verknüpfung zwischen dem Zeichen, dessen

> Sinne und dessen Bedeutung ist der Art, dass dem Zeichen ein bestimmter Sinn und diesem wieder eine bestimmte Bedeutung entspricht, während zu einer Bedeutung (einem Gegenstande) nicht nur ein Zeichen zugehört.

What did Russell require of an explication of the statement that, say, "the centre of mass of the Solar System" denotes the centre of mass of the Solar System? A difficulty with this question is that he did not succeed in giving what we would now consider an explication; he only attempted to fulfil the following precondition of an explication; in order to explain such a statement, we first have to show that we can talk about meanings, just as we can talk about denotations. This was all he actually did. Because, as we shall see, Russell did not succeed in speaking about meanings otherwise than with the help of inverted commas, he stopped at the conclusion that "the whole distinction of meaning and denotation has been wrongly conceived" (Russell 1905b, p. 487). This implied that the puzzle about George IV - who wished to know whether Scott was the author of Waverley, but did not wish to know that Scott was Scott - could not be solved with an appeal to such a distinction. Russell's new solution was, of course, that "when we say, 'George IV wished to know whether Scott was the author of Waverley,' we normally mean 'George IV wished to know whether one and only one man wrote Waverley and Scott was that man'", though "we may also mean: 'One and only one man wrote Waverley, and George IV wished to know whether Scott was that man'" (Russell 1905b, p. 489).

Russell thought that he could show that a theory which assumes both denotations and meanings was incoherent because "we cannot succeed in both preserving the connexion of meaning and denotation and preventing them from being one and the same" (Russell 1905b, p. 486). His demonstration rested mainly on the lemma that "the meaning cannot be got at except by means of denoting phrases" (Russell, 1905b, p. 486). The weak point is, however, that he did not explain what is wrong with this. He referred to "the first line of Gray's Elegy" with the expression 'the denoting complex occurring in the second of the above instances'. This expression denotes "the first line of Gray's Elegy" in the sense in which a phrase is said to denote a denotation, and the denoting complex occurring in the second of the above instances denotes the first line of Gray's Elegy in the sense in which a meaning is said to denote a denotation. The denoting complex occurring in the second of the above instances therefore denotes the line which consists of the words 'the', 'curfew', 'tolls', 'the', 'knell', 'of', 'parting' and 'day'. It is true that we do hereby have a meaning by means of a denoting phrase, namely 'the denoting complex occurring in the second of the above instances' but we did not mention this phrase, we used it. So it seems that the crucial denoting relation here is not "linguistic through the phrase".

Admittedly we encounter a new problem, namely that the expression 'the denoting complex occurring in the second of the above instances' itself has a meaning. Where are we to find this denoting complex which is to denote the denoting complex occurring in the second of the above instances? (Cf. Russell 1905b, p. 487.) The answer, that it is the denoting complex occurring in the sentence underlined above, leads to an infinite regress. Russell, however, did not show that this infinite regress causes any harm, but to me this infinite regress does not appear to be logically impossible (cf. Russell 1903a, par. 476, on "Meaning and indication" in Frege's "Ueber Sinn und Bedeutung").

Consider now the example which helped Russell to establish his conclusion (Russell 1905b, p. 486):

> ... let "C" be "the denoting complex occurring in the second of the above instances".
> Then
> C = "the first line of Gray's Elegy",
> and the denotation of C = The curfew tolls the knell of parting day. But what we meant to have as the denotation is "the first line of Gray's Elegy". Thus we have failed to get what we wanted.

If Russell wanted to denote the _complex_ "the first line of Gray's Elegy", he could have either _used_ the expression "the denotation of the expression 'the denoting complex occurring in the second of the above instances'", or he could have _used_ the expression 'the denoting complex occurring in the second of the above instances'. In the latter case he would have referred to the meaning of a denoting phrase. (If Russell wanted to denote the phrase 'the first line of Gray's Elegy', then he could have used the expression 'the denoting phrase occurring in line 8 on page 486 of _Mind_, _NS_ 14'.)

Russell was aware of this possibility when he wrote (1905b, p. 487):

> Thus to speak of C itself, _i.e._ to make a proposition about the meaning, our subject must not be C, but something which denotes C.

Indeed, that which denotes the meaning "the first line of Gray's Elegy" is the expression 'the denoting complex occurring in the second of the above instances'. This expression itself also has a meaning, namely, "the denoting complex occurring in the second of the above instances".

We can also use the expression '"the first line of Gray's Elegy"' when we want to speak of the meaning; this expression denotes the meaning, as Russell would agree. But unfortunately, in the last paragraph of his alleged refutation of the distinction of meaning and denotation he concentrated on this expression. He considered "C" - that is ""the first line of Gray's Elegy"" and "the first line of Gray's Elegy" - and found their relation "wholly mysterious". Instead, he should have considered the meaning of the expression 'the denoting complex occurring in the second of the above instances'.

We may say:

The denoting complex occurring in the second of the above instances is

itself a denoting complex.
"The denoting complex occurring in the second of the above instances" is also a denoting complex.

The last mentioned denoting complex, or in Russell's terminology, the second of the two denoting complexes occurring in the just-given example, is precisely the denoting complex which is to denote the denoting complex occurring in the second of the above instances, or "the first line of Gray's Elegy".

I conclude that Russell failed "to prove that the whole distinction of meaning and denotation had been wrongly conceived". Nevertheless nothing in his discussion points in the direction of "ontological doubts". It is possible that he had difficulties with the fact that the theory which makes the distinction implies that meanings are themselves also denotations (Russell 1905b, p. 486):

> But the difficulty which confronts us is that we cannot succeed in <u>both</u> preserving the connexion of meaning and denotation <u>and</u> preventing them from being one and the same;

<u>This</u> difficulty disappears, however, as soon as one realizes that meanings are themselves denotations <u>of other meanings</u>. What is so strange, is that Russell himself saw this (1905b, p. 487):

> Thus it would seem that "C" [for example: "the denoting complex occurring in the second of the above instances" - H.V.] and C [the denoting complex occurring in the second of the above instances - H.V.] are different entities such that "C" denotes C;

However, he added (o.c.):

> but this cannot be an explanation because the relation of "C" to C remains wholly mysterious.

He may have meant that the relation between, say, "the denoting complex occurring in the second of the above instances" and the denoting complex occurring in the second of the above instances is, again, merely linguistic through the phrase. But the relation between the second of the two denoting complexes occurring in the third of the above instances and the denoting complex occurring in the second of the above instances is <u>not</u> linguistic through the phrase. Here it is a relation between two denoting complexes. This might trouble someone who thought that it was a relation between a denoting complex -such as "the first line of Gray's Elegy" and something which is not a denoting complex. The right thing to say, of course, is that (1) a denoting complex itself can be a denotation and (2) not every denotation is a denoting complex. In a sense this asks for a modification of the general principle that meanings and denotations are not on the same level. But the conclusion that meanings and denotations are one and the same is not justified. (Even an example of a denoting complex denoting itself would not show that the distinction between meanings and denotations was wrongly conceived.)

It seems that Russell concentrated too much on the representation of meanings with the help of inverted commas. It is quite possible to talk about meanings without such things, but instead in terms of expressions containing the words 'the denoting complex'. If Russell had realized this more explicitly, he could have directed his criticism against such representations by pointing out that they also take meanings for granted. In contrast, his own theory had at least two advantages. It accounted for some intuitions about meanings, and it escaped a crucial problem connected with Frege's suggestion that the meaning of a denoting phrase contains the way in which the denoted object is given. For example, part of what one can have in mind when one is talking about the meaning of the expression 'the first line of Gray's Elegy' might be expressed by the words 'whatever is not preceded by any line of Gray's Elegy, and precedes all other lines of Gray's Elegy'. This can easily be said in Russell's theory, even in a situation in which

Frege's suggestion does not work, namely when there is no such thing as the first line of Gray's Elegy.

I am now in a position to take a stand on Hochberg's claims (cf. p. 183–184):

(1*) Russell showed only a partial understanding of Frege on "sense and reference"; he did not take in account Frege's view on the difference in levels of meanings and denotations.

(2*) The crucial formulation that "the meaning denotes the denotation" can be traced back to Frege's essay "Ueber Sinn und Bedeutung". Russell's examples are such that the assimilation of his earlier views with Frege's makes sense.

(3*) Russell's arguments are indeed directed against Frege, with the proviso formulated under (1), in short:

(4*) Russell's statement of Frege's view is incomplete.

(5*) Russell's argument is defective because he failed to see that meanings can themselves be denoted (by meanings) in another way than "merely linguistic through the phrase". However, Russell's argument can be improved by pointing out that this other way clarifies nothing.

Theory of denoting and theory of knowledge

"On denoting" has only two paragraphs on theory of knowledge, but they contain four strong claims. The first three are the following:

(1) All thinking has to start from acquaintance.

(2) In perception we have acquaintance with objects of a more abstract logical character, but

(3) we do not necessarily have acquaintance with the objects denoted by phrases composed of words with whose meanings we are acquainted.

These claims were formulated in a paragraph preceding Russell's exposition of his 1905 theory of denoting. They played no role in the argumentation for that theory and can still be understood in terms of the theory given in The principles of mathematics. As a matter of fact, the latter theory could provide an explanation for the third claim. (This will be shown presently.) But this does not rule out that the claims themselves were new in Russell's philosophy. Here for the first time, as far as I can see, Russell worked with a rather broad relation of (immediate) acquaintance. How broad cannot be immediately understood from the above rendering of the claims. The centre of mass of the Solar System is excluded from acquaintance, as well as "other people's minds" (Russell 1905b, p. 479-480). It can be easily admitted that we cannot perceive the centre of mass of the Solar System; we now learn that we also have no acquaintance with this point "in thought". Nevertheless, "we can affirm propositions about it", and this presents a problem for theory of knowledge. It was formulated in general terms at the end of the first of the two paragraphs in question (Russell 1905a, p. 480):

> All thinking has to start from acquaintance; but it succeeds in thinking about many things with which we have no acquaintance.

Russell's adoption of propositions from the theory of logic as the objects of beliefs in theory of knowledge now becomes important: an analysis of propositions - presumably "about" things with which we have no acquaintance - within theory of logic might contribute to a solution of the above problem in theory of knowledge. A theory of denoting along the lines of The principles of mathematics could yield the following account: knowing the meaning of a denoting phrase (such as 'the centre of mass of the Solar System') or the corresponding denoting concept is a necessary condition for understanding a proposition about a thing with which we have no acquaintance but which we describe by means of the denoting phrase; furthermore, knowing that there is (exactly) one object which is denoted by the meaning or the denoting concept is a necessary condition for knowing such a proposition to be true.

By the abolishment of denoting concepts in "On denoting", such answers to the above problem were no longer available. Propositions which were formerly said to have denoting concepts as their constituents now had to be analyzed as propositions without such constituents. Instead, they contained the constituents expressed by the words of the denoting phrase (cf. Russell 1905b, p. 492). The question of how we can think about things with which we have no acquaintance could now be answered with the help of the fourth claim, stated in the second of the two paragraphs on theory of knowledge (Russell 1905b, p. 492):

(4) Thus in every proposition that we can apprehend (i.e. not only in those whose truth of falsehood we can judge of, but in all that we can think about), all the constituents are really entities with which we have immediate acquaintance.

How broad is this claim: do we need immediate acquaintance with the so-called logical constants out of which the propositions of logic and mathematics are built? Then we cannot escape a realistic interpretation of the entities with which these disciplines are concerned, and Russell's views on the distinction between logic and mathematics on the one hand, and theories on the world of existence on the other hand - set out in "The existential import of propositions" - can no longer be maintained. However, there is no indication that Russell was thinking of mathematical propositions or logical constants when he formulated this fourth epistemological claim. As yet, the propositions considered in the paragraph in question seem to be concerned only with "such things as matter (in the sense in which matter occurs in physics) and the minds of other people"; "we know them as what has such and such properties". Apparently, they have properties of which we have (immediate) acquaintance; but the relation of acquaintance remained unspecified. We are still far away from the philosophical views of the Russell of The problems of philosophy. (In this work, Russell interpreted "subsistence" or "being" as being timeless; the entities which have being are now called universals. Examples are "whiteness" and "two". An arithmetical statement, such as "two and two are four" now deals exclusively with universals, "and therefore may be known by anybody who is acquainted with the universals concerned and can perceive the relation between them which the statement asserts" (Russell 1912a, p. 164). It was not before 1910 that Russell engaged in this kind of metaphysics. An explicit avowal of a kind of realism was given after the completion of the first volume of Principia mathematica, in a paper on the philosophical implications of mathematical logic. See the last section of this chapter.)

The doctrine of false abstractions; classes and propositions

Russell's new theory of denoting had a considerable impact on his further logical treatment of the principles of mathematics, by Russell in collaboration with Whitehead. Led by the insight that paradoxes are apt to result from the (wrong) assumption that each word or phrase has an independent meaning, Russell invented a so-called "substitutional theory of classes and relations" which seemed to solve the contradictions found in logic. But Russell was afterwards not satisfied with this theory and abandoned it. The crucial question which occupies me in this section is whether ontological considerations moved Russell to do so. This was posited by Grattan-Guinness in Dear Russell - dear Jourdain. A commentary on Russell's logic, based on his correspondence with Philip Jourdain. I shall argue that Grattan-Guinness was right in a more limited sense than he intended. The argument will consist of three steps:

(1) Russell's so-called doctrine of false abstractions which led directly to the substitutional theory does not involve ontological considerations.
(2) In Russell's work on the correspondence theory of truth, ontological considerations were put forward with respect to propositions.
(3) Russell's correspondence theory of truth had decisive impact on his logical theory.

If I am right, then Russell did become involved in ontological considerations in logical theory, although this fact has nothing to do with his rejection of the substitutional theory. Already in "On denoting", Russell pointed out that the theory could be used in mathematics for the interpretation of definitions by means of denoting phrases. Shortly afterwards, he realized that the method might be extended from the so-called denoting phrases to class expressions and number expressions. This looked promising, for now there was a chance that the contradictions discovered by Burali-Forti and Russell could be solved.

Russell's argumentation seems to have been as follows: Expressions such as 'the present king of France' and 'the present king of England' have no meaning in isolation; a meaning can only be assigned to propositions in whose verbal expression they occur. Now propositions apparently about classes and numbers have to be interpreted in a similar way, only one must not assign a meaning to every statement containing class- or number expressions. One has to see to it that reasonings which lead to contradictions cannot occur.

In general, the procedure for dealing with class expressions along the lines of the procedures for definite descriptions, amounted both to a generalization and to a modification of the approach in "On denoting". This can be seen from Russell's paper "On the substitutional theory of classes and relations". The generalization was embodied in the following "fundamental logical principle": "in any sentence, a single word, or a single component phrase, may often be quite devoid of meaning when separated from its context". The modification was such that statements containing a certain word may only have a meaning when the word occurs in the <u>proper</u> context. Outside such contexts, we have statements "totally devoid of meaning" or "nonsensical" in the sense that they are "phrases which do not express propositions at all" (Russell 1973a, p. 166). Examples of such statements are, according to Russell, 'the number one is bald', 'the number one is fond of cream cheese', and, especially, such a statement as 'the class of human beings is a human being'.

The problem, of course, is an explication of what has to count as a <u>proper</u> context. Russell's strategy was such that his representation of class symbols by "incomplete symbols" automatically excluded improper contexts. This can be seen as follows. The incomplete symbols for classes were so-called matrices of the form p/a for: 'the result of replacing a in p by'. They occur in phrases of the form p/a;x, in which the semicolon separates the matrix from its argument. Such phrases are part of meaningful sentences such as a sentence of the form 'p/a;x is true for all values of x'. For example:
'the number of members of the class of all snakes in Ireland is 0'
becomes:
'for all values of x, Socrates is a snake in Ireland/Socrates;x is false'.
The incomplete symbols for classes of classes are matrices of a more complicated form $q/(p,a)$, in which q is a statement containing the class expression p/a.

Matrices of the form p/a are called "of the first type"; of the form $q/(p,a)$ "of the second type" and so on. In this way, cardinal numbers, interpreted as classes of classes of entities become matrices of the second type; ordinal numbers, as classes of relations of entities, become matrices of the third type. Thus a hierarchy arises, which eventually solves Burali-Forti's problem (Russell 1973a, p. 177; cf. p. 183-184):

> Suppose now we have a series of such ordinals: what will be the type of its ordinal number? This is a class of relations between such ordinals. Each ordinal is of the third type; hence a relation of two of them is of the sixth type, and a class of such relations is of the seventh type. Thus the ordinal number of a series of ordinals applicable to series of entities is a matrix of the seventh type.

The solution of Russell's paradox is especially interesting (Russell

1973a, p. 172):

> To say that x is a member of the class α is now to say that for some values of p and a, α is the matrix p/a and $p/a;x$ is true. Here, instead of the variable function φ, which could not be detached from its argument, we have the two variables p and a, which are entities, and may be varied. But now 'x is an x' becomes meaningless, because 'x is an α' requires that α should be of the form p/a, and thus not an entity at all. In this way membership of a class can be defined, and at the same time the contradiction is avoided.

Apparently, propositions are "entities" in the sense that they are values of a variable x in a propositional function φx. This use of the term 'entity' was explained by Russell in his exposition of "the gist of Frege's theory" (Russell 1973a, p. 171). But at the end of the paper, when Russell commented on this double aspect of the substitutional theory - that classes are not "entities", whereas propositions are - it may be doubted whether he held to this use of the term 'entity' (Russell 1973a, p. 188):

> All that is obtained by the substitutional method would still be true if there were after all such entities as classes and relations; we do not deny that there are such entities, we merely abstain from affirming that there are. The only serious danger, so far as appears, is lest some contradiction should be found to result from the assumption that *propositions* are entities; but I have not found any such contradiction, and it is very hard to believe that there are no such things as propositions, or to see how, if there were no propositions, any general reasoning would be possible.

The question whether "there are" such entities as classes can easily be interpreted as an ontological question. But notice that Russell's standpoint of an agnostic logician avoided any conclusion whatsoever.

Russell also put forward purely methodological considerations when he admitted that the technical development of the principles of mathematics was rendered "much more complicated" by the substitutional theory, though he said that there was "really a simplification" as regards the fundamental assumptions of the theory (Russell 1973a, p. 188).

Why did Russell abandon the substitutional theory? Was it only for technical reasons, or did he have "reservations about the substitutional theory" because of "its commitment to propositions as abstract objects", as Grattan-Guinness suggested (Grattan-Guinness 1977a, p. 92). In the latter case, analytical results would have been rejected on ontological grounds of a sort. However, a closer inspection of Russell's development towards <u>Principa mathematica</u> shows that the purpose of the doctrine of "false abstractions" was not an alleged dispensing with "abstract objects". This is not to say that extra-logical arguments were completely absent in Russell's application of this doctrine to propositions. The following account of some of Russell's writings from 1906 to 1910 shows how this came about. Starting point was his attempt to solve certain paradoxes concerning propositions. In "Les paradoxes de la logique", the paradox about the man who says 'I am lying' in the sense of 'There is a proposition which I am affirming and which is false' was dealt with by using the word 'proposition' in a special sense, by confining it to "what is affirmed by a statement containing no apparent variable" (Russell 1973a, p. 207). Then it follows that the man's statement is <u>false</u>, because what he is affirming is not a proposition. (One might think that the paradox reappears if the man says 'I am now making a statement which is false' but Russell rejects this "because there is no way of speaking of statements in <u>general</u>: we can speak of statements containing one, two, three ... apparent variables, but not of statements in general" ...)

This theory seems rather <u>ad hoc</u>, but Russell also had an argument independent of the solution of what he called vicious-circle fallacies (Russell 1973a, p. 207):

> If I say, 'Socrates is mortal', there is a fact corresponding to my assertion, and this fact is what I will call the proposition. I assume that there is such a thing as the proposition even in cases where it is false, but not in cases where it is general.

A statement of the form 'For all values of x, ...x...' was said to be "an ambiguous statement of any of the various propositions" of the given form '...x...'. In such a case we have, according to Russell, "merely an unlimited undetermined choice among a number of propositions". The assertion of an existential statement such as 'I met a man' would amount to the same as an assertion of "some one of the propositions of the form 'I met x, and x is human', without in any way deciding as to which one I assert". This view is certainly not an articulated theory - but it makes no use of arguments from extra-logical theories. Russell simply thought that "the paradoxes besetting logistic" could be attributed to one source: vicious circles, which arise "where a phrase containing such words as all or some (i.e. containing an apparent variable) appears itself to stand for one of the objects to which the words all or some are applied" (Russell 1973a, p. 213).

In order to solve vicious circle paradoxes, Russell required a "restatement of logical principles". He found it in an extension of the method applied to denoting phrases, given in "On denoting" (Russell 1973a, p. 213):

> The difficulty is, that there is reason to hold that all must be capable of meaning absolutely all; thus the phrases in question must not stand for entities at all. This result we secured; in the case of statements, by saying that a statement about all things states an ambiguous proposition

> about any one among things, and in the case of classes and relations, by saying that these are to be regarded as merely verbally or symbolically parts of statements, not as parts of the facts expressed by the statements in question.

An explanation why Russell used the word 'fact' instead of 'proposition' for the formulation of his extension of the principle that denoting phrases do not stand for genuine constituents of propositions in whose verbal expressions they occur, might be that he had just restricted the use of the word 'proposition' to what is expressed by sentences without apparent variables. It is not clear, however, to what extent Russell realized that this way of speaking could have awkward consequences for the formulation of his theory of denoting. For he now had to say that a sentence such as 'Scott was the author of Waverley' - even in its fully analyzed form, 'It is not always false of x that ...' - does not express a proposition. As a matter of fact, he remarked in a footnote that his use of the word proposition was "proposed solely for the purposes of the present discussion": "elsewhere it would probably prove inconvenient" (Russell 1973a, p. 207).

Russell's new use of the word 'proposition' was very restricted indeed. This can be shown with the help of his own example of the law of excluded middle, given in the form 'every proposition is either true of false'. It is not enough to remark that this law is itself not a proposition in the above restricted sense - since it contains an apparent variable - but a true statement in the sense that "all the propositions which the statement ambiguously denotes are true (in the previous sense)" (Russell 1973a, p. 208). For apart from the question in which sense a propostion could be said to be true, the law of excluded middle certainly does not state that only statements without apparent variables are either true or false. Russell admitted this when he distinguished different laws of excluded middle, according to the number of apparent variables of the statements to which they apply. But he did not give any indication how, given the restricted law, we can "infer a new law of excluded middle applying to statements with one apparent variable" and "then go on to three, four, ... apparent variables". In this respect Principia mathematica was a real advance, though at the cost of a considerable number of distinctions. The word 'proposition' was brought back to its original use while at the same time a hierarchy of propositions was established. The law of excluded middle could be given in the form

$$\text{"} \vdash . p \vee \sim p \text{"}$$

without any restriction upon p, provided the negation and disjunction are given a suitable meaning appropriate to the order in the hierarchy of the propositions involved. That is to say, Russell took the disjunction "$p \vee q$" of what are called "elementary propositions" as a primitive idea and defined disjunctions such as "$p . \vee . (x) . \varphi x$". As a matter of fact, the notion of "elementary proposition" appeared to be one of the central ideas of the new approach, comparable to the role of "propositions" in the substitutional theory. But whereas the propositions of the latter theory were considered "to be there" or to be "entities", the propositions of the theory of types were only "incomplete symbols".

Why did Russell give up the substitutional theory? It is true that the paper "On the substitutional theory of classes and relations" was not accepted for publication by the London Mathematical Society, but Russell might after all have been dissatisfied with the theory (cf. Lackey's remarks in Russell 1973a, p. 130). Grattan-Guinness traced at least some of Russell's reasons for abandoning the substitutional theory back to the circumstance that he "was developing a correspondence theory of truth at this time, and (...) found it difficult to postulate the existence of objective falsehoods which false propositions could name or in which one could have a belief" (Grattan-Guinness 1977a, p. 92).

It is perhaps no accident that a commentator anno 1977 speaks about a logical theory as if the acceptance of "propositions" as values of variables would commit one to the existence of objective falsehoods. But what concerns me here, is the question whether Russell himself had similar views. If so, it is possible that Russell's abandonment of a logical theory was influenced by extra-logical arguments. How decisive was Russell's work on "the nature of truth and falsehood" for Principia mathematica? Why did he include a version of a correspondence theory into the discussion of the theory of logical types?

It can be argued that Russell required of the logical theory of Principia mathematica that it could deal successfully with logical paradoxes, and - since he considered paradoxes involving the notion of truth "logical paradoxes" - had to give a solution of such paradoxes by logical means. This demanded an explication of the notion of truth and falsehood. We shall see that Russell gave an explication within the context of theory of knowledge, which he used for his theory of logic. It was here that he adduced arguments derived from the nature of belief or judgment and perception, though strictly speaking these arguments are dispensable from a purely technical point of view.

The account just given tallies with Russell's report to Jourdain that consideration of the paradox of the liar and its analogues had led him "to be chary of treating propositions as entities" (Grattan-Guinness 1977a, p. 105). Even so, the Epimenides paradox can be seen to be concerned with such a proposition as "all the judgments made by Epimenides are false" and this requires an analysis of judgments. Indeed, Russell reached his new standpoint - that a phrase which expresses a proposition is an "incomplete symbol" - through just such an analysis.

According to this analysis, "a judgment does not have a single object, namely the proposition, but has several interrelated objects" (Russell 1973a, p. 224):

> That is to say, the relation which constitutes judgment is

> not a relation of two terms, namely the judging mind and the proposition, but is a relation of several terms, namely the mind and what are called constituents of the proposition.

Subsequently Russell assumed that an analogous analysis could be given for other statements in which a proposition appears as one of the subjects. This would explain why such a statement as "$\{(x).\varphi x\}$ is a man" is meaningless (Russell 1973a, p. 230), though Russell did not go further than to remark that in this statement the proposition cannot be broken up into its constituents. (This is not very informative if we know only that a proposition is "not a single entity, but a relation of several".) In any event, Russell's analysis of judgments must be considered an essential part of the argument why propositions are not entities. The question is now whether this analysis makes use of extra-logical arguments.

The analysis in question is first mentioned in the earlier essay on the nature of truth (Russell 1906c). It is here confronted with another analysis, which acknowledged "facts" and "objective falsehoods" as the objects of beliefs, in order to justify, for example, why people who believe that the sun goes round the earth seem to believe <u>something</u>. This analysis is akin to Russell's position in his critique of Meinong, though it is not clear whether he saw it as a logical view, only indirectly related to theory of knowledge. But the new analysis was directly connected with theory of knowledge by its reference to perception and belief (Russell 1906c, p. 45):

> When we entertain a correct belief, that which we believe may be called a <u>fact</u>. A fact is always complex: thus when we perceive that something exists, the something is not a fact, but its existence is a fact. If A exists, "A's existence" is a fact; perception consists in the apprehension of such facts. Similarly 2+2 is not a fact; but it is a fact that 2+2=4. Given any related objects, these objects in relation form a complex object, which may be called a fact; and when

> we apprehend this fact, we have knowledge. Truth, then, we might suppose, is the quality of beliefs which have facts for their objects, and falsehood is the quality of other beliefs. And a fact may be defined as whatever there is that is complex.

At the time, Russell had not yet arrived at the so-called "relational theory" that a judgment consists of a multiple relation. However, in answer to the old objection that "the man who judges falsely, undoubtedly thinks, and thinks something", Russell said that in this case "it is thinking of the objects of the ideas which constitute the belief". Judgment would be a kind of "discursive knowledge". Moreover, the view that a belief is a complex of ideas, not a single idea, had "the merit of distinguishing between the perception of a fact and the judgment which affirms the same fact": "in perception, the actual object is before the mind, in the belief there is merely a complex of presentations of constituents of the objective complex, these presentations being related in a manner corresponding to that in which the constituents of the objective complex are related" (Russel 1906c, p. 47) [37].

The decisive step was taken in the seventh essay "On the nature of truth and falsehood" in Russell's Philosophical essays. This essay replaced the above-mentioned essay on the nature of truth. The change of title is significant: one of the reasons for the rejection of the propositional theory of judgment was that this theory "leaves the difference between truth and falsehood quite inexplicable". But as it happened, there was also an ontological reason, as Russell pointed out in Meinongian terms (Russell 1910a; 1966a, p. 152):

> If we allow that all judgments have objectives, we shall have to allow that there are objectives which are false. Thus there will be in the world entities, not dependent upon the existence of judgments, which can be described as objective falsehoods.

The relational theory, on the other hand, was given as an account of a cognitive act, judging, particularly with respect to a so-called relational judgment, such as "A loves B". This judgment was considered a relation between the person judging and A, B and the relation "love" together with a direction, called the "sense" of the relation, establishing that it "proceeds" from A to B rather than from B to A. The judging relation is a so-called multiple relation: it can relate a mind with several items, in the above example three, in other cases two, or four or more. We are not told how we have to conceive this relation if, say, a person judges that there are matches inside a match-box. (This example is Moore's, who complained that Russell made things unduly simple: Moore 1962a, p. 25-28). Moreover, the condition that, in the relational case, the relation must be "before the mind" as "proceeding from A to B rather than from B to A" is far from clear. (As a matter of fact, an early criticism by Stout on this point forced Russell to reword his account: "in the act of judging ArB, the sense must be confined to judging, and must not appear in the r" (Stout 1910a, p. 203).)

It was this relational theory which had an impact on Russell's logical theorizing. How the transition from theory of knowledge took place can be seen from the following decomposition of Russell's line of thinking:

(1) Perception is a relation of two terms, the percipient and the object of perception. The latter is perceived as one object.
(2) Attention may show that the object of perception is complex; we then judge, for example that a and b stand in the relation R.
(3) The judgment that a and b stand in the relation R is a relation of four terms, the judging subject, a, b and R.
(4) Such a judgment is said to be <u>true</u> when there is a complex "a-in-the-relation-R-to-b", and is said to be <u>false</u> when this is not the case.
(5) A supposed unique object of a judgment is a false abstraction, because a judgment has several objects, according to (3).

(6) It follows that such a phrase as 'a has the relation R to b', a phrase which is said to express a proposition, is an "incomplete symbol": it requires some supplementation in order to acquire a complete meaning.

The last conclusion establishes a position similar to that of so-called denoting phrases: a phrase 'the so-and-so' which is said to express an object, is also an "incomplete symbol". This is a logical result, so we may assume that (6) is also a logical result. This is in accordance with Russell's formulation in "The theory of logical types" and Principia mathematica I (Russell 1973a, p. 224-225):

> Owing to the plurality of the objects of a single judgment it follows that what we call a 'proposition' (in the sense in which this is distinguished from the phrase expressing it) is not a single entity at all.

But this logical position was reached without a logical analysis of the kind in which statements containing a definite description are treated. Theory of knowledge had become relevant for logical theory. The notion of "entity" was no longer free from ontological commitment as to what there is in the world - to borrow Russell's phrase in "On the nature of truth and falsehood". It is not surprising then that other notions, such as "individual" and "elementary proposition" gave rise to extra-logical considerations in Principia mathematica. It seems that Grattan-Guinness was right in questioning Lackey's (unqualified) statement that Russell was "certainly not confused" in ontological questions (Russell 1973a, p. 134; Grattan-Guinness 1977a, p. 93, n. 1).

I conclude this section with some remarks on the question of classes. By abandoning the substitutional theory, Russell had to take a new stand towards this problem. As we know, he decided to treat class expressions along the same lines as definite descriptions. Thus, class expressions were introduced in "Mathematical logic as based on the theory of types" as incomplete symbols.

This treatment is interesting from a methodological standpoint, because Whitehead and Russell, perhaps for the first time, stated a so-called criterion of adequacy for their definition-in-use of "the class defined by the function $\varphi \hat{z}$". They enumerated "five requisites which a definition of classes must satisfy" in order to "recommend" their definition. They also showed that this definition satisfied these requisites (Whitehead and Russell 1910a; 1927a, p. 76).

From then on, they sometimes used the term 'reconstruction' for their approach, thereby indicating that a mere "analysis" of mathematics is not sufficient because of the presence of contradictions. I argued in Part Two that Russell's theory of classes as such was not an <u>ontological</u> reconstruction. This is now confirmed by the fact that the requisites do not involve ontological connsiderations.

For example, the first two requisites demand that every propositional function must <u>determine</u> a class - "which may be regarded as the collection of all the arguments satisfying the function in question" - and that two propositional functions which are formally equivalent must <u>determine</u> the same class. So in a sense there are classes, and Russell and Whitehead could state their fourth requisite - the third is the converse of the second - as follows:

> In the same sense in which there are classes (whatever this sense may be), or in some closely analogous sense, there must also be classes of classes.

Indeed, the number 1 was defined as the class of unit classes, so Whitehead and Russell could state that "without classes of classes, arithmetic becomes impossible" (1910a; 1927a, p. 77).

The problem of classes is, of course, that without further distinctions the first four requisites would yield a class of all classses satisfying the function $\alpha \notin \alpha$. So the last requisite was that under all circumstances it must be meaningless to suppose a class identical with one of its members. This was secured by the theory which attributed different meanings to class symbols in different contexts, under the guidance of the theory of types.

In general, Whitehead and Russell considered the result of their definition such that the way in which they used classes corresponded in general to the use in ordinary thought and speech (cf. 1910a; 1927a, p. 24); from this they concluded that it did not anticipate any extra-logical interpretation of classes, because "whatever may be the ultimate interpretation of the one (of the above-mentioned ways) is also the interpretation of the other". There are more statements in this tenor, for example (o.c.):

> We have avoided the decision as to whether a class of things has in any sense an existence as one object. A decision of this question in either way is indifferent to our logic, though perhaps, if we had regarded some solution which held classes and relations to be in some sense real objects as both true and likely to be universally received, we might have simplified one or two definitions and a few preliminary propositions.

It is true that the last part of this quotation seems to suggest the possibility of a correspondence between a logical notion of entity and an extra-logical notion of "real object", but this is still different from the idea that the theory of classes of <u>Principia mathematica</u> implied that "there are classes" in any sense. On this matter, Whitehead and Russell wrote that it was not necessary for them "to assert dogmatically that there are no such things as classes" (1910a; 1927a, p. 72); again:

> It is only necessary for us to show that the incomplete symbols which we introduce as representatives of classes yield all the propositions for the sake of which classes

might be thought essential. When this has been shown, the mere principle of the economy of primitive ideas leads to the non-introduction of classes except as incomplete symbols.

"The philosophical implications of mathematical logic"

Russell ended his "Mathematical logic as based on the theory of types" with the remark that the theory of types raised a number of difficult philosophical questions concerning its interpretation. He then said to prefer stating the theory without reference to such questions, "leaving these to be dealt with independently" (Russell 1956a, p. 102). Though this was not done, Russell made some philosophical remarks in "La théorie des types logiques" and in Principia mathematica. Some of these remarks seem important enough for consideration, since it can be argued that they had a considerable impact on Wittgenstein's early philosophy and helped in shaping Wittgenstein's and Russell's new philosophy of logic.

The notions of "elementary proposition" and "individual" are central, but there is also a discussion of the so-called logical constants in another paper by Russell on the philosophical implications of mathematical logic, which appeared first in French in 1911. This subject was touched upon by Wittgenstein in his letter to Russell of June 22, 1912. His suggestion "that there are no logical constants" proved to be an important one and returned in the "Logisch-Philosophische Abhandlung".

In this section, the most remarkable of Russell's philosophical comments are brought together to provide some valuable background information for the next section.

We saw that the notion of proposition was connected with Russell's account of the nature of perception and judgment. Indeed, the "definition" of truth and falsehood was based on a philosophical view, expressed in the following quotation (Whitehead and Russell 1910a;

1927a, p. 43):

> The universe consists of objects having various qualities and standing in various relations. Some of the objects which occur in the universe are complex. When an object is complex, it consists of interrelated parts. Let us consider a complex object composed of two parts *a* and *b* standing to each other in the relation *R*. The complex object "*a*-in-the-relation-*R*-to-*b*" may be capable of being perceived; when perceived, it is perceived as one object. Attention may show that it is complex; we then judge that *a* and *b* stand in the relation *R*. Such a judgment being derived from perception by mere attention, may be called "judgment of perception".

This view presented Russell with the ingredients for a definition of truth and falsehood of so-called elementary judgments:

> In fact, we may define truth, where such judgments are concerned, as consisting in the fact that there is a complex corresponding to the discursive thought which is the judgment. That is, when we judge "*a*-in-the-relation-*R*-to-*b*", our judgment is said to be true when there is a complex "*a*-in-the-relation-*R*-to-*b*", and is said to be false when this is not the case.

The first of the above quotations suggests that what normally would be called a "fact" is now called a complex object, but in the second quotation facts reappear. But what is the difference between judging that *a* has the relation *R* to *b* and judging that there is a complex "*a*-in-the-relation-*R*-to-*b*"? And what is the difference between the complex object "*a*-in-the-relation-*R*-to-*b*" and the fact that there is a complex *a*-in-the-relation-*R*-to-*b*?

Such questions were not posed by Russell, who took the above definition of elementary truth as a starting point for definitions of truth and falsehood of non-elementary judgments. The purpose of the procedure was twofold: it solved the Epimenides paradox and it justified, among others, the definition of the disjunction between a non-elementary proposition such as $\forall x \varphi x$ and an elementary proposition, in terms of the disjunction of elementary propositions [38].

Clearly, the notion of elementary judgment and the connected notion of elementary proposition are very fundamental for the system of Principia mathematica. In fact, the notion of "elementary proposition" was taken to be one of the undefined notions, also called primitive ideas. It was explained by Whitehead and Russell without any philosophical connotation as a proposition "which does not involve any variables or, in other language, one which does not involve such words as 'all', 'some', 'the' or equivalent for such words" (1910a; 1927a, p. 91). Elementary propositions as such contain "no reference, explicit or implicit, to any totality". However, it was also said that "the clearest instances of propositions not containing apparent variables are such as express immediate judgments of perception" such as "this is red", where this is something given in sensation ... (1927a, p. 50).

It is not clear from the text of Principia mathematica how important the last remarks are. Are they only contributions to the elucidations of the primitive notion of "elementary proposition"? There is one remarkable passage which casts doubt upon this (1927a, p. 44-45):

> But take now such a proposition as "all men are mortal". Here the judgment does not correspond to one complex, but to many, namely "Socrates is mortal", "Plato is mortal", "Aristotle is mortal", etc. (For the moment, it is unnecessary to inquire whether each of these does not require further treatment before we reach the ultimate complexes involved. For purpose of illustration, "Socrates is mortal" is here treated as an elementary judgment, though it is in fact not one, as will be explained later. Truly elementary judgments are not very easily found.)

This comment seems to imply that the logical theory of <u>Principia mathematica</u> can only be <u>applied</u> to statements describing "ultimate complexes", or complex things in the universe which are not further (ontologically) analysable. Apart from such applications, <u>Principia mathematica</u> would present only "pure logic" in which the analysis of mathematical theories can be carried out using only the primitive ideas of logic. This indeed seems to have been Russell's view when he wrote that "no constant elementary proposition will occur in the present work, or can occur in any work which employ only logical ideas" and that the ideas and propositions of logic are all <u>general</u> (1927a, p. 93). This is not to say that "pure logic" has no connection with "some world of individuals" (o.c.):

> Thus, giving the name "individual" to whatever there is that is neither a proposition nor a function, the proposition "every individual is identical with itself" or the proposition "there are individuals" will be a proposition belonging to logic. But these propositions are not elementary.

How harmless is all this? Is it not very easy to understand the phrase "whatever there is" in the first quotation of this section as being concerned with "the universe"? In that case, the axiom of infinity for example, implying that there are infinitely many <u>individuals</u>, would be an hypothesis about the existing world. And indeed, Russell would write in 1911 that "the axiom of infinity is purely empirical" and that "it is possible <u>a priori</u> that ν be the number of individuals in the universe" - where ν is some finite or transfinite number (Russell 1973a, p. 254). Earlier, in "Mathematical logic as based on the theory of types", Russell wrote that "if any one chooses to assume that the total number of individuals in universe is (say) 10367, there seems no <u>a priori</u> way of refuting this opinion". Now this remark can still be seen as an incidental one, not implying that the axiom of infinity had an "empirical" character, but the following argument given by Russell in 1911 leaves no room for misunderstanding (1973a, p. 254):

> But according to empirical evidence, and the divisibility of finite objects, it seems artificial to suppose that there are a finite number of objects in the universe.

Here is an argument in favour of the axiom of infinity which would not be unbecoming of an ontological reconstruction. It is true that Russell said also that "it is sufficient to demonstrate that the finitist hypothesis is much more difficult and less simple than the other" and concluded that it was better to presume that the number of individuals is infinite - thus giving in fact only methodological arguments; but the suggestion cannot be denied that the logical system of Principia mathematica implies the existence of objects of some kind. How powerful this suggestion was, will be shown in following sections. For the moment, I am only presenting texts which anticipate later discussions in "philosophical logic".

Another view on pure logic was developed by Russell in "The philosophical implications of mathematical logic" (1911). Starting point here was the old doctrine of The principles of mathematics that pure mathematics is "entirely hypothetical" in the sense that its propositions are of the form "If any subject satisfies such and such a hypothesis, it will also satisfy such and such a thesis" (1973a, p. 289). It has as a consequence that in pure mathematics "we have never to discuss facts that are applicable to such and such an individual object; we need never know anything about the actual world". Pure mathematics and pure logic are only concerned with so-called logical constants. But whereas in The principles Russell said about these logical constants that "they are to be defined only by enumeration" (1903a, p. 10), he now offered a kind of explication and gave some comments on "logical truths". The explication of the notion of a logical constant amounts to the following: any part of a proposition which cannot be generalized with the help of variables is a logical constant.

Given examples are the relation of membership of a class (*is-a*), the universal quantifier (*all*) and the material implication (*if, then*). Russell considered them "purely formal concepts" and this presented him with another characterization of logical constants: "those which constitute pure form". This already sounds like a philosophical interpretation. And indeed, Russell said that logic and mathematics force us to admit "a kind of realism in the scholastic sense". It is true that this view did not influence the actual shape of the logico-mathematical theory, for this theory was inspired by "the usual scientific motives of economy and generalization"; but the philosophical afterthoughts did effect a conspicuous change in terminology, as is shown by The problems of philosophy. There Russell reckoned the logical constants to a world of universals which was said to subsist or have being, where "being" is opposed to "existence" as being timeless. That is to say, logical constants belong to the same ontological category as sensible qualities, relations of space and time, and similarity. They differ with these universals only in being more "abstract", but they can also be known by acquaintance and one can have intuitive a priori knowledge about them. This knowledge is embodied in what might be called "self-evident truths" and among such truths are included "those which merely state what is given in sense, and also certain abstract logical and arithmetical principles, and (though with less certainty) some ethical propositions" (Russell 1912a, p. 171).

CHAPTER EIGHT

WITTGENSTEIN'S PHILOSOPHY

Introduction

The appearance of a young philosopher, Ludwig Wittgenstein, marks the beginning of a new period in analytic philosophy. Russell had just finished the analysis of mathematics and under the influence of Whithehead went on to revise his philosophical position in reconstructionist style. He also worked on some problems in theory of knowledge with special atttention to psychological issues. Wittgenstein convinced him that "philosophical logic" is the right approach for the modern philosopher: a field of research in which the "true" problems of philosophy could be solved (cf. Wittgenstein 1974a, p. 14).

At first, Russell did not integrate this new approach fully into his work: the second chapter "Logic as the essence of philosophy", of his book Our knowledge of the external world is independent of the other chapters. But the situation is different in the 1918 lectures on the philosophy of logical atomism. In these lectures, Russell used Wittgenstein's suggestions in such a way that almost all his philosophical activities were combined into one kind of philosophy. The guiding idea of a "logically perfect language" appears to be very important for the development of what would later be called "ideal language philosophy", a mixture of logic and ontology which will occupy us in a subsequent section.
Wittgenstein completed his "Logisch-Philosophische Abhandlung" in 1918. It too has been seen as a forerunner of ideal language philosophy. I shall argue in the next two chapters that both Wittgenstein and Russell, despite differences, conflated logic or logical analysis and the task of giving a general description of the universe. This conclusion is not at all new; many authors came to the same conclusion. But I shall try to give a detailed analysis of how the conflation came about, as independent as possible of later views which themselves can

be seen to be influenced by the conflation.

Wittgenstein's "Logisch-Philosophische Abhandlung" is a short and obscure book (cf. Wittgenstein 1974a, p. 71). Many subjects are discussed, but mostly in such a way that it is very difficult to see what the author might have had in mind. Moreover, singling out separate parts may misrepresent the author's intentions, because of the evident connections of the several claims. I shall make use of a bifurcation which Wittgenstein himself made, the distinction between "saying" and "showing", as a guideline to approach Wittgenstein's complicated philosophy. I discuss Wittgenstein's theory of logic and his so-called picture theory. The chapter closes with a discussion of Wittgenstein's views about the natural sciences - paradigm: mechanics - on the one hand, and logic and mathematics on the other hand, and with their relative position (gegenseitige Stellung). It will be argued that in Wittgenstein's philosophy the two programs in analytic philosophy are brought together in such a way that logical analysis and ontological reconstruction cannot be considered distinct.

The doctrine of saying and showing

Wittgenstein began his philosophical career by disagreeing with some of Russell's views as he found them in Principia mathematica. In his letters to Russell we find the following dissenting remarks (Wittgenstein 1974a, p. 10, p. 19, p. 23):

(1) "there are no logical constants" (22.6.1912)
(2) "every theory of types must be rendered superfluous" (1.1913)
(3) "objection to your theory of judgment" (6.1913)

He developed a kind of doctrine in which (1) and (2) are explained and, as to (3), an alternative theory of judgment is given. The general idea of this doctrine is formulated in a subsequent letter to Russell (Wittgenstein 1974a, p. 71). There Wittgenstein says that the main point of his "Logisch-Philosophische Abhandlung" was "the theory of what can not be expressed (...) but only shown (gezeigt)". As I see it, this distinction indeed provides the clue to a plausible interpretation of much of what Wittgenstein wrote before 1919. It goes back at least to the notes which Wittgenstein dictated to Moore in April 1914. These begin with the remark that "logical so-called propositions shew logical properties of language and therefore of Universe but say nothing" (Wittgenstein 1961a, p. 107). Every "real" proposition, it is said, is a proposition which does say something, but also shows something about the universe, or "mirrors some logical property of the Universe". The expression 'says' (sagt) was deliberately chosen: it establishes a relation between symbolic situations and real situations. Let me explain.

Consider a notation of a certain chess move, say '1.e2-e4'. That here the expression '1.' is followed by the expression 'e2-e4', says that the first move is the change in place of the pawn on the field e2 to the field e4. Of course, the notation does not itself say that the expression '1.' is followed by the expression 'e2-e4'; it exhibits or shows this. In order to say this, one must have recourse to another symbolism, say English: "The expression '1.' is followed by the expression 'e2-'e4'.". The official symbolism for chess moves doesn't have the means for saying such things. In general, each specific symbolic system has properties not describable in that system. But consider now a symbolic system in which everything that can be the case in the universe can be described. Then there are, according to Wittgenstein, "properties" of this universal system which cannot be described in the system itself: they can be called "logical properties" in order to distinguish them from "ordinary" or "real" properties for which the symbolic system has suitable symbols. Wittgenstein illustrates such a logical property by the property of the system that a certain kind of symbol symbolizes an object and not a property or relation; it is shown in certain features of the symbolism.

On Wittgenstein's view, the above doctrine immediately explains why (1) there are no logical constants, and (2) a theory of types is superfluous. To begin with the latter, an articulated theory of types would have to talk about things, properties, relations and their difference; but formulations such as "that a certain symbol of a universal symbolism stands for a thing" are nonsensical; they are attempts to describe logical features of the symbolism which can only be shown. Such words as 'thing' and 'properties' do not signify ordinary concepts; they stand for so-called formal concepts; their expression is a feature of certain symbols (Wittgenstein 1921a, 4.126). Similarly, the logical connectives as an example of logical constants correspond with so-called operations on sentence-forms; they establish internal relations between structures of sentences. Their "properties", laid down in truth conditions, cannot be described with sentences of the universal symbolism itself.

The distinction between what can and cannot be expressed in a symbolic system is not new: Frege made use of it in par. 13 of Begriffsschrift; Wittgenstein's novelty was the application of the distinction to "a language which can express or say everything that can be said". This gives a new content to the old philosophical distinction between "formal" and "material"; the same holds for the distinction between "internal" and "external" relations. This can already be seen from the notes dictated to Moore (Wittgenstein 1961a, p. 115-116); here we also find an interesting example of a reinterpretation of a Kantian way of speaking: Wittgenstein makes a suggestion on how one might make sense of the assertion that "logical laws are forms of thought and space and time forms of intuition" (o.c., p. 117). This will be discussed in the context of Wittgenstein's account of so-called a priori propositions.

Wittgenstein's doctrine of saying and showing also contains the clue to his alternative for Russell's theory of judgment. We have seen that Wittgenstein criticized Russell's original analysis for the reason that it could not distinguish between judging something meaningful and judging nonsense: judging was considered a multiple relation between a judging subject and constituents of what was formerly considered a proposition, but propositions had disappeared. In his unpublished

"Theory of knowledge", Russell introduced logical forms as constituents of judgment-complexes, and this made them a kind of entity. However, the fact that the logical form of a simple atomic complex can not be considered another constituent of that complex, prevents it from being an entity. Wittgenstein's doctrine avoids such an incoherence, but he soon realized that the logical form of a proposition must play a role in judgments (cf. Wittgenstein 1979a, p. 106).

In his notes dictated to Moore, Wittgenstein compared a statement of the form 'I believe (that) such and such is the case' with "(the statement) 'such and such is the case <u>says</u> (that) such and such is the case". Unfortunately, he forgot to indicate in which respect such statements are comparable. In my view, the comparison concerns the that-clauses: the expression 'that the first move is the change in place of the pawn on the field e2 to the field e4' has the same (logical) role in 'Wittgenstein believes that the first move is the change in place of the pawn on the field e2 to the fiels e4.' as in 'That the expression '1.' is followed by the expression 'e2-e4' <u>says</u> that the first move is the change in place of the pawn on the field e2 to the field e4.'. What matters here, of course, is not the truth-value of the sentence 'The first move is the change in place of the pawn on the field e2 to the field e4.', but its <u>sense</u>, that is, what this sentence shows: how things stand when it is true. (Cf. Wittgenstein 1921a, p. 213; 4.022.) This seems to imply that not only the statement has the same logical form as the described state of affairs, but that this also holds for "a part of the judging subject", more precisely, for his thought that such and such is the case. This is a remarkable example of how one can draw conclusions about the structure of facts from what is seen as the logical form of statements. (This evaluation is compatible with Wittgenstein's concluding remark of the notes that "it is just as impossible that *I* should be a simple as that "p" should be" (Wittgenstein 1961a, p. 118; cf. Wittgenstein 1921a, 5.5421). That a thought consists of "psychical constituents that have the same sort of relation to reality as words" was stated by Wittgenstein in his letter of 19.8.1919 to Russell (Wittgenstein 1974a, p. 72).)

Theory of logic and picture theory

The above outline of some aspects of Wittgenstein's new approach to Russellian problems leads smoothly to the conclusion that, for Wittgenstein, logic is somehow dependent on world-descriptions: it deals with these "logical properties" of the Universe which a language describing the Universe "mirrors"; and "logical so-called propositions shew in a systematic way those properties" (Wittgenstein 1961a, p. 107). For, according to Wittgenstein, pictures and the pictured must have something in common in order that the one can be a picture of the other at all (Wittgenstein 1921a, 2.161), namely the (logical) form of reality (o.c., 2.18). Logical forms are forms of (possible) facts; suppose that "there are" elementary sentences of the subject-predicate form; then this means that "there are" subject-predicate facts (cf. Wittgenstein 1961a, p. 2-4), though neither of these things can be said: "Die Frage nach der Existenz eines formalen Begriffes ist unsinnig. Denn kein Satz kann eine solche Frage beantworten" (Wittgenstein 1921a, 4.1274). But the assimilation of logical forms with features of the Universe is unmistakably present.

This is, of course, rather abstract, and does not as such imply that logical theory is shaped by extra-logical considerations. Did Wittgenstein not say that logic must take care of itself in the sense that "the rules of logic" are only "syntactical rules for manipulation of symbols" (Wittgenstein 1961a, p. 116)? Wittgenstein indeed often stressed the importance of what he called logical syntax; for example, in the "Logisch-Philosophische Abhandlung": "Wenn wir die logische Syntax irgend einer Zeichensprache kennen, dann sind bereits alle Sätze der Logik gegeben" (Wittgenstein 1921a, 6.124). And logical syntactical rules ought to be formulated without recourse to what the symbols denote (Wittgenstein 1921a, 3.33):

> In der logischen Syntax darf nie die Bedeutung eines Zeichens eine Rolle spielen; sie muss sich aufstellen lassen, ohne

> dass dabei von der <u>Bedeutung</u> eines Zeichen die Rede wäre, sie darf <u>nur</u> die Beschreibung der Ausdrücke voraussetzen.

Wittgenstein held that neither Frege nor Whitehead and Russell considered semantics when formulating their logical theories (Wittgenstein 1961a, p. 2; 22.8.1914):

> Wenn sich syntaktische Regeln für Funktionen <u>überhaupt</u> aufstellen lassen, dann ist die ganze Theorie der Dinge, Eigenschaften etc. überflüssig. Es ist auch gar zu auffällig, dass weder in den "Grundgesetzen" noch in den "Principia Mathematica" von dieser Theorie die Rede ist. Nochmals: denn die Logik muss für sich selbst sorgen.

As is clear from earlier sections, Wittgenstein was not completely right in his evaluation of <u>Principia mathematica</u>. His remark in the "Logisch-Philosophische Abhandlung" that Russell's theory of types required the denotation of the signs, was nearer to the truth. But this is not the essential point. What matters is that from Wittgenstein's doctrine of saying and showing it follows that semantical rules in which words as 'thing' and 'property' occur, are <u>nonsensical</u>, because they try to express what can not be said but only shown. This makes it clear <u>how</u> logic has to do with world descriptions; the latter provide <u>presuppositions</u> for the former. An explicit statement of this decisive principle was given in the "Logisch-Philosophische Abhandlung" (Wittgenstein 1921a, 6.124):

> Die logischen Sätze beschreiben das Gerüst der Welt, oder vielmehr, sie stellen es dar. Sie "handeln" von nichts. Sie setzen voraus, dass Namen Bedeutung, und Elementarsätze Sinn haben: und dies ist ihre Verbindung mit der Welt. Es ist klar, dass es etwas über die Welt anzeigen muss, dass gewisse Verbindungen von Symbolen - welche wesentlich einen bestimmten Charakter haben - Tautologien sind. Hierin liegt das Entscheidende.

Wittgenstein's terminology is Fregean, but there are important differences, as can be seen from his outline of an adequate universal symbolism. Only names have a denotation; each name represents an object in the context of a proposition. An elementary proposition does not name or denote anything, it expresses a "thought", that is a "logical picture" of an atomic fact (or elementary situation, state of affairs). The terms 'object' - Gegenstand, Ding - and 'fact' - Tatsache - have to be interpreted in an exclusively realistic sense: "Die empirische Realität ist begrenzt durch die Gesamtheit der Gegenstände. Die Grenze zeigt sich wieder in der Gesamtheit der Elementarsätze (Wittgenstein 1921a, 5.5561). Elementary propositions are fundamental for all other kinds of propositions (cf. Wittgenstein 1921a, 4.411). Thus, when p and q are two elementary propositions, then a non-elementary proposition such as $(p \supset q)$ is defined as a so-called truth function of p and q, assigning to each pair of the so-called truth possibilities of p and q a certain truth-possibility ("true" or "false"). A non-elementary proposition is called a tautology when the truth possibility "true" is always assigned, and a contradiction when "false" is always assigned.

Wittgenstein's idea was that each non-elementary proposition which is not a tautology or contradiction "restricts" the truth-possibilities of the constituent elementary propositions, in the sense that it "excludes" those n-tuples of truth-possibilities of the elementary propositions that are assigned the truth-possibility "false". Hence for any two elementary propositions p and q, $(p \supset q)$ excludes that p is true and q is false. This is, of course, nothing new for someone who read Frege's Begriffsschrift. But Wittgenstein went further by interpreting the expressions 'der Umstand dass' and 'ist eine Tatsache' realistically. In his view, for any two elementary propositions p and q, $(p \supset q)$ "says" that it is not the case that the situation described by p belongs to the world and the situation described by q does not belong to the world. On the other hand, tautologies say "nothing", for they "allow" each possible elementary situation. They are not "pictures of reality". The same holds for contradictions; these allow no possible elementary situation (Wittgenstein 1921a, 4.462).

Wittgenstein believed that it was also possible to define sentences of the form $\exists x f x$ or $\forall x f x$ as a kind of function of elementary propositions, for example by stipulating that $\exists x f x$ "says" that it is the case that the situation described by an elementary proposition of the form fx belongs to the world, or, for short, expresses that there is a true proposition fx. From this it follows that, for example, $(fa \supset \exists x f x)$ is always assigned the value "true" [39].

Compared with Frege's Begriffsschrift, what is new in this approach is only the realistic turn. This is evidenced by Wittgenstein's remark that it must show something about the world that certain combinations of symbols - which essentially have a definite character - are tautologies. According to Wittgenstein the symbol a in the formula $(fa \supset \exists x f x)$ denotes a certain object in the world and the occurrence of the sign f also symbolizes a certain future of the world. (The last formulation will be clarified on the following pages.)

The part of Wittgenstein's philosophy concerned with the form of elementary propositions remained in a rudimentary state. Commentators found it very difficult to reconcile such statements in the "Logisch-Philosophische Abhandlung" as 4.22: "Der Elementarsatz besteht aus Namen. Er ist ein Zusammenhang, eine Verkettung, von Namen.", 4.24: "Den Elementarsatz schreibe ich als Funktion der Namen in der Form: "fx", "$g(x,y)$", etc.", and 5.5261: "Ein vollkommen verallgemeinerte Satz ist, wie jeder andere Satz zusammengesetzt. (Dies zeigt sich daran, dass wir in "$(\exists x,\varphi).\varphi x$" "$\varphi$" und "$x$" getrennt erwähnen müssen. Beide stehen unabhängig in bezeichnenden Beziehungen zur Welt, wie im unverallgemeinerten Satz.)". The problems do not become any easier when we encounter in Wittgenstein's notebooks remarks such as "Auch Relationen und Eigenschaften etc. sind Gegenstände" (Wittgenstein 1961a, p. 61). Yet some account of Wittgenstein's ideas about possible compositions of elementary propositions seems desirable when we want more insight into the "presupposition" of logical propositions mentioned in the quotation on p. 224 (6.124).

This brings me to a closer inspection of Wittgenstein's picture theory (Bildtheorie): this theory, being directly applicable to elementary propositions, talks about "elements of the propositional sign" (Elemente des Satzzeichens, Wittgenstein 1921a, 3.2). Fortunately, Wittgenstein himself gave an explanation of a (presumably elementary) proposition (Wittgenstein 1921a, 3.1432; cf. Wittgenstein 1961a, p. 105):

> Nicht: "Das komplexe Zeichen 'aRb' sagt, dass a in der Beziehung R zu b steht", sondern: Dass "a" in einer gewissen Beziehung zu "b" steht, sagt, dass aRb.

It is easy to see why the first formulation is not a correct one: what "says" something is a symbolic fact; the fault with the notation 'aRb' is that it does not describe a symbolic fact. It is an attempt to name what cannot be named at all (cf. Wittgenstein 1921a, 3.144), whereas in Wittgenstein's picture theory it is crucial that facts can only be symbolized by facts (cf. Wittgenstein 1921a, 2.16; 1979a, p. 97). The fact that a certain object stands in a certain relation to another object can be symbolized by the fact that a certain mark on the paper stands in a certain relation to another mark on the paper.

Suppose that we give a similar account for the proposition fa: that the sign a has a certain property says that fa, or more precisely that the denotation of a has the property indicated by the presence of the sign f. Then it can be asked which property the sign a possesses; considering the two-dimensional "fact" created by the printed figure, we might say that the sign a has the property of being immediately to the right of the sign f, or, perhaps, the property of replacing the letter x in fx. But is this essential? It is conceivable that a different medium is used; can we not symbolize that an object has a certain property by a tone with a certain pitch (or, better, a tone having a certain pitch)? If so, then this is, according to Wittgenstein, only possible because the two different symbolizations share what he called the same logical form of representation. What this logical form of representation actually is cannot be said, but only shown. To stay within the language of the metaphor of what can only be shown: this logical form is "seen" by anyone who understands the proposition or the musical signal (cf. Wittgenstein 1961a, p. 109). Of course, this logical form is not a constituent of an understanding complex, as Russell once thought. There is a logical connection (logische Verbindung) between the sign *a* and the sign *f*, which is itself not an entity; it has an important function: it makes the sentence a "logical picture" of an elementary situation (das logische Bild eines Sachverhaltes: o.c., p. 25).

It is significant that the sign *f* occurs in *fa* and not, say the sign *g*. One might say that the presence of the sign *f* indicates the presence of a certain property, in the sense that the "fact" that the name *a* has the property of being in a certain way logically connected to the sign *f* or *fx*, symbolizes that the denotation of *a* has the property indicated by the presence of the sign *f*. Here we have a kind of coordination of properties. Similarly, there is a coordination of relations in Wittgenstein's paradigmatic case: that the sign *a* and the sign *b* stand in the relation of being in a certain way logically connected to the sign *R* symbolizes that the denotation of *a* and the denotation of *b* stand in the relation indicated by the presence of the sign *R*. Both *fa* and *aRb* are "logically articulated" (logisch gegliedert) and that makes them pictures of an elementary situation (cf. Wittgenstein 1961a, p. 8; Wittgenstein 1921a, 4.032).

However, Wittgenstein was not completely satisfied with his own account, for he uttered the following complaint on 15.4.1915: "Ich kann eben nicht herausbringen, in wie fern der Satz das Bild des Sachverhaltes ist! Beinahe bin ich bereit, alle Bemühungen aufzugeben" (Wittgenstein 1961a, p. 41). It is interesting to follow Wittgenstein's attempts to gain more insight into this and related problems. Even the old (Russellian) "complexes" again became subject of discussion (o.c., p. 48). Wittgenstein realized that a reification of such complexes is possible, even when this would be "nothing but a logical manipulation" (o.c., p. 49). This brought him to reexamine the notion of "simple object" (einfacher Gegenstand). Gradually the idea emerged that the notion of simplicity is relative: by giving an object a simple sign (einfacher Zeichen), the object "figures" (fungiert) as a simple object in the sense that its composition becomes completely irrelevant with respect to possible logical consequences. This explains why the logic of, say, Principia Mathematica may quite well be applied to "ordinary propositions" (Wittgenstein 1961a, p. 69):

> z.B. aus "Alle Menschen sind sterblich" und "Socrates ist ein Mensch" folgt nach dieser Logik "Socrates ist sterblich", was offenbar richtig ist, obwohl ich, ebenso offenbar, nicht weiss, welche Struktur das Ding Socrates oder die Eigenschaft der Sterblichkeit hat. Diese fungieren eben hier als einfache Gegenstände.

The idea of the relativity of what can be considered an object accords nicely with Wittgenstein's view that different systems of world description are possible, though some are "finer" than others. (Cf. Wittgenstein 1961a, p. 35; Wittgenstein 1921a, 6.341, 6.342.) But this does not affect the principle that logic is connected with world descriptions. On the contrary, logic must even be such that it can be applied to statements describing the world with the help of "an infinitely fine network" (cf. Wittgenstein 1961a, p. 39; Wittgenstein 1921a, 5.511).

Some commentators have argued that only the so-called things of the "Logisch-Philosophische Abhandlung" are objects, and that properties and relations are not - contrary to the above quotation. It is true that Wittgenstein was not very careful in his use of the words 'Ding' and 'Gegenstand', but it was explicitly stated in the Notebooks that relations and properties too are objects (cf. Wittgenstein 1961a, p. 53: "Die Namen sind notwendig zu einer Aussage, dass dieses Ding jene Eigenschaft besitzt u.s.f."). We must not forget that, according to

Wittgenstein, there are different logical kinds of objects or different logical forms of objects. Each object of a certain logical kind can be indicated in one or another way, but the kind of connection depends on the kind of object: "Wenn ein Name einen Gegenstand bezeichnet, so steht er damit in einer Beziehung zu ihm, die ganz von der logischen Art des Gegenstandes bedingt ist und diese wieder charakterisiert" (Wittgenstein 1961a, p. 70; the distinction between different logical forms of objects was also mentioned in a discussion of mathematical physics: "Aber wie merkwürdig: in den bekannten Lehrsätzen der mathematischen Physik erscheinen weder Dinge noch Funktionen noch Relationen noch sonst logische Gegenstandsformen!! Statt der Dinge haben wir da Zahlen, und die Funktionen und Relationen sind durchweg rein mathematisch!!" o.c., p. 66).

It has been remarked by commentators that the view that properties and relations are also objects cannot be reconciled with Wittgenstein's statements that (1) the elementary proposition consists of names, (2) names are indicated by single letters x, y, z and (3) the elementary proposition is written as "function of names" in the form fx, $\varphi(x,y)$, etc. From this it indeed does not follow that, for example, fa represents an elementary situation consisting of two objects, for this would imply that f is also a name, contradicting (2). On the other hand, it must be admitted that f is an element of the propositional sign fa; therefore it must in some or another way contribute to the representation of the elementary situation in question. How is this possible?

My standpoint is implicit in the above account of how the propositional sign fa represents. This sign does not "consist" of two names, one being the name of an object in the narrow sense (<u>Ding</u>), the other being the name of a property. It "consists" of one name, a, having a certain property; the sign fx contributes to this property. Again, that the sign a has this property symbolizes that the denotation of a has a certain property. However, the sign fx is not a name of this property <u>within the context of the elementary proposition</u> fa, though it might be said that it "renders" this property (cf. Wittgenstein 1921a, 4.126: "Die formalen Begriffe können ja nicht, *wie die eigentlichen Begriffe, durch eine Funktion dargestellt werden*" - my italics). That is not to say that the property in question cannot be treated as an object in some other situation. It might be that we want to say that the property itself has a certain (non-logical) property, or stands in a certain (non-logical) relation with another property. Then the property figures (*fungiert*) as a simple object and we can name it. We might even use the sign f for this object. In any case it must be possible to replace this sign by a variable, as we saw before in $(\exists x, \varphi)\varphi x$. This use of variables carries "ontological commitment" with it: "Nun aber wenden wir *Variable* an, das heisst, wir reden sozusagen von den Urbilden allein, ganz abgesehen von irgend welchen einzelnen Fällen. Wir bilden das Ding, die Relation, die Eigenschaft vermittelst Variablen ab und zeigen so, dass wir diese Ideen nicht aus gewissen uns vorkommenden Fällen ableiten, sondern sie irgendwie a priori besitzen" (Wittgenstein 1961a, p. 65).

In my view of the structure of elementary propositions in the "Logisch-Philosophische Abhandlung", Wittgenstein closely followed Frege's doctrine of the possibility of interpreting sentences as a function of arguments. (Cf. Wittgenstein 1921a, 4.24; Wittgenstein 1971a, p. 116.) (We saw that he did not dissociate himself from Frege's reading of $\Phi(A)$ as "A has the property Φ" (Frege 1879a, par. 10; Wittgenstein 1969a, p. 69, on 'Socrates ist sterblich'.)) New in Wittgenstein's treatment was first a "theory of symbolism" holding that "facts are symbolized by facts" in the following sense: "that a certain thing is the case in the symbol says that a certain thing is the case in the world" (Wittgenstein 1961a, p. 105). Second, he took the notion of "world" which occurred in this theory literally. Third, only statements describing the world have sense or "can be said" (Wittgenstein 1921a, 6.53). Other sentences are either nonsensical, or "without sense", attempting to say what can only be shown; the idea underlying sentences of the latter kind is that a language with which the world can be described has certain "formal properties" coinciding (!, on the ground of the theory of symbolism) with "logical properties" of the world (Wittgenstein 1921a, 6.12, 6.34).

Wittgenstein and ontological reconstructionism

"Only statements describing the world have sense or can be said, but they show something as well". It was this insight that had such a decisive impact on Wittgenstein's attitude towards logical analysis, because he now had to distinguish between statements in need of analysis which "describe the world" and those which "show the logic of the world". In the first case, the analysis is concerned with empirical statements of natural science (cf. Wittgenstein 1921a, 4.11; also 6.53); in the second case, the subject matter is formed by so-called a priori "knowledge", treated by Russell in The problems of philosophy, including not only logical and mathematical propositions, but also synthetic a priori propositions of Kantian philosophy (Hertz!), material a priori truths of the phenomenological kind (Russell!) and general principles of mathematical physics (Mach!). We might be inclined to discuss the cases in turn; but we must realize that they belong together, just as saying and showing act in concert.

A "correct symbolism" would be such that everything which can be said could be expressed in it, whereas everything which can be shown - and therefore cannot be said - would be mirrored in it. Such a correct symbolism would embody ontological commitment on the basis of Wittgenstein's realistic turn. Does this mean that Wittgenstein can be considered an ontological reconstructionist? In this section I deal with two answers to this question:

(1) Wittgenstein took a general position in the reconstructionist sense; he opts for a kind of physicalism, though he did not pronounce a preference for a specific reconstruction in the style of Whitehead.
(2) Wittgenstein required of a correct symbolism that the philosophical problem of the status and characterization of so-called a priori propositions would be solved by it, so he

presented a kind of prolegomena to a possible logical analysis of such propositions. But he did not reach the position of formal ontological reconstructionism in the sense that he shows how, for example, certain "logical properties" of time can be mirrored in a correct symbolism.

In his early reflections on philosophy, Wittgenstein was already explicitly oriented towards the natural sciences (including psychology); philosophy was considered "the doctrine of the logical form of scientific propositions": the word 'philosophy' had to mean something which stands above or below the natural sciences (cf. Wittgenstein 1979a, p. 1921a, 4.111). Eventually he wrote that the totality of true propositions was the same as the totality of the natural sciences. It seems to follow that if the task of the logical analysis of scientific statements is to be performed successfully, a logical symbolism is needed in which the analysis can be carried out. And indeed, the Wittgenstein of the "Logisch-Philosophische Abhandlung" alluded more than once to a sign language (Zeichensprache) which obeys the rules of logical grammar or syntax (Wittgenstein 1921a, 3.325); he also said that Frege's and Russell's formalism was such a sign language, though not yet a "correct" one - among others things because it was still possible to write pseudo-propositions in it such as $a=a$ or $\exists x.x=a$ (cf. Wittgenstein 1921a, 5.534).

What might a correct formalism look like? Wittgenstein believed that it was possible to remove the identity sign wholly from Russell's notation: identity could be indicated merely by the identity of signs (Wittgenstein 1961a, p. 19, p. 34; cf. Wittgenstein 1921a, 5.53 - 5.5352). He criticized Russell's definition of '=' by arguing that the statement that two numerically different objects have all properties in common, represents a possible state of affairs (Wittgenstein 1921a, 5.5302; cf. 2.202 and 2.221 for the use of the word 'Sinn'). This is a very interesting argument, because it confirms my claim that Wittgenstein interpreted the notion of object (thing) realistically - objects are constituents of states of affairs. It can now be understood why Wittgenstein originally considered the statement $\exists x . x=x$ a proposition of physics, and said the same, not only of the axiom of infinity of Principia mathematica, but also of the axiom of reducibility (Wittgenstein 1974a, p. 39, in a letter to Russell, end of 1913).

As we shall see in the next chapter, Russell took this criticism seriously and devised the formulation that "pure logic, and pure mathematics (which is the same thing), aims at being true, in Leibnizian phraseology, in all possible worlds" (Russell 1920a, p. 192). The notion of "possible world" was also employed by Wittgenstein in his notebooks, though in a remark which was explicitly not intended to be part of the "Logisch-Philosophische Abhandlung" (Wittgenstein 1961a, p. 83).

Such formulas as $\exists x . x=x$, therefore, can not be logical propositions; but Wittgenstein would consider the problem of eliminating them from logical theory solved, as soon as the identity sign had been removed (Wittgenstein 1921a, 5.535). However, it cannot have escaped Wittgenstein's attention that Whitehead and Russell needed their axiom of infinity as a hypothesis in order to be able to deal with infinite ("inductive") cardinal numbers (cf. Wittgenstein 1961a, p. 10-11). So if Wittgenstein really wanted to justify mathematical reasoning within a Russellian formalism, he would have to find some principle fulfilling the job of the axiom of infinity. Since he did not do this, it is difficult to see how his account of mathematics can be supposed to cover the whole of mathematics (cf. Ramsey, 1923a, p. 282; I review Ramsey's view on this subject below).

Given Wittgenstein's symbolism, the analysis of statements of natural science requires a means of representing physical situations: names having a (physical) denotation and elementary propositions having a (physical) "sense" mean that they represent logically possible situations. If this condition is fulfilled, the result of a "logical analysis" of a scientific statement at the same time presents an "ontological analysis" of a physical situation. That is to say, each "completely analyzed" statement is such that "to the objects of the thought which it expresses correspond elements of the symbolic sentences" (Wittgenstein 1921a, 3.2; 3.201ff). In addition to this, the sentence shows - thanks to the "correct" formal system - the logical structure of reality (o.c., 4.121). To sum up: "in the proposition there must be just as much to distinguish as in the state of affairs which it represents" (o.c., 4.04); "to a definite logical combination (Verbindung) of signs corresponds a definite logical combination of their denotations" (o.c., 4.466).

This identification of logical analysis with ontological analysis is itself interesting enough, and can be seen to have led to the view that philosophy is a doctrine of "the logical forms of facts", as Russell formulated it, and that the application of logic is concerned with the question what elementary situations there are (cf. Wittgenstein 1921a, 5.557). But at the time that Wittgenstein wrote his logico-philosophical treatise, there existed a program of formal ontological reconstructionism (see Part Two). Is there a connection between this kind of analytic philosophy and Wittgenstein's conception of philosophy?

My answer to the last question is that such a connection was indeed present. Wittgenstein was certainly interested in different systems of world description and discussed "the relative position of logic and mechanics" (Wittgenstein 1961a, p. 35: 6.12.1914; Wittgenstein 1921a, 6.342). His discussion started with an explication of the method of symbolizing in his formalism as a "system of co-ordinates which maps the elementary situation into the proposition" (Wittgenstein 1961a, p. 20: 29.10.1914). He also wrote that "one might conceive two co-ordinates a_p and b_p as a proposition stating that the material point p is to be found in the place (ab): for this statement to be possible, the co-ordinates a and b must really determine a place" (o.c., p. 20-21). Or, returning to statements of his symbolism (or any symbolism in general): "Damit eine Aussage möglich ist, müssen die logischen

Koordinaten wirklich einen logischen Ort bestimmen!" (o.c., p. 21; cf. p. 38). This way of speaking was retained in the "Logisch-Philosophische Abhandlung" (among others: Wittgenstein 1921a, 3.41), and the above view on formulas of mechanics was not merely incidental. Newtonian mechanics was said to bring "the description of the world to a unified form" (Wittgenstein 1961a, p. 35: 6.12.1914; 1921a, 6.341); other systems of description of the world determined "forms of description" in the sense that each of them says that "all propositions in the description of the world must be obtained in a given way from a number of given propositions" - the axioms of the mechanics in question (o.c.). Apparently, a necessary condition for this is the logical analysis of the propositions of physics in terms of the given system: "Wie man mit dem Zahlensystem jede beliebige Anzahl muss hinschreiben können, so muss man mit dem System der Mechanik jeden beliebigen Satz der Physik hinschreiben können. Und hier sehen wir nun die gegenseitige Stellung von Logik und Mechanik" (o.c.).

One might be tempted to conclude that different systems of mechanics - as sketched by Whitehead in his memoir "On mathematical concepts of the material world" present as many different <u>ontological</u> views of the world, reflecting different philosophical positions. However, this idea does not seem to have occurred to Wittgenstein within the context of mechanics; the only mentioned differences concern degrees of completeness and of simplicity (Wittgenstein 1961a, p. 35-36; 1921a, 6.342). Nevertheless he can be seen to have shown some interest in such questions when he remarked in his notebooks that Hertz's "invisible masses are admittedly (<u>eingestandenermassen</u>) pseudo-objects" (Wittgenstein 1961a, p. 36). Wittgenstein also mentioned "the two theories of heat, the one conceiving heat as a stuff, the other as a movement" as "a characteristic example" of his "theory of the significance of the physical description of nature" (<u>Theorie der Bedeutung der physikalischen Naturbeschreibung</u>) (Wittgenstein 1961a, p. 37). This seems to imply that Wittgenstein preferred <u>physical</u> (ways of) world descriptions above other kinds. If this was indeed the case, then we can understand why Wittgenstein considered Russell's method in his

"Scientific method in philosophy" simply a step backward from the method of physics (Wittgenstein 1961a, p. 44), namely, because he did not like Russell's reconstruction of physical objects in terms of sense-data. Why else would he have written: "Die Zerlegung der Körper in materielle Punkte, wie wir sie in der Physik haben, ist weiter nichts als die Analyse in einfache Bestandteile"? (Wittgenstein 1961a, p. 67).

As it appears, Wittgenstein's interpretation of the formula $\exists x.x=x$ as a sentence of physics was a first sign of a physicalism which was manifest in the orientation towards the natural sciences in the "Logisch-Philosophische Abhandlung". But there are further indications in this direction. Talking about "the differences in structure" between the colour red and the colour green, Wittgenstein said that physics arranges them in a series: "Und nun sieht man, wie hier die wahre Struktur der Gegenstände ans Licht gebracht wird" (Wittgenstein 1961a, p. 81; 1921a, 6.3751). I conclude that Wittgenstein thought that the physicalist reconstruction of (our knowledge of) the external world was the right one, though he did not take a stand towards different ways of carrying out such a reconstruction. In any case, physicalism proved to be useful in part of Wittgenstein's treatment of a priori propositions. This subject deserves a closer inspection but before coming to it, I want to discuss possible repercussions of Wittgenstein's physicalism on psychological issues.

It cannot easily be derived from the "Logisch-Philosophische Abhandlung" or from the notebooks how Wittgenstein's physicalistic orientation influenced his view on psychological issues. It is of course easy to state that for Wittgenstein psychology was one of the natural sciences. But did this standpoint have a demonstrable effect on his views on perception, thinking and willing - the subjects which Wittgenstein discussed? I believe that these views are at least not at variance with a combined biophysical psychophysical appraoch. Let us see how this opinion is supported by the texts on the mentioned issues.

Problems in the field of perception are treated by Wittgenstein in the "Logisch-Philosophische Abhandlung" within the context of "propositional forms of psychology" (Satzformen der Psychologie). That is, a statement of the form 'I perceive that such and such is the case' was compared with 'I believe that such and such is the case'. Wittgenstein considered Russell's view that perception is a relation of the percipient and a complex object of perception incorrect; "To perceive a complex means to perceive that its constituents are related in such and such a way" (Wittgenstein 1921a, 5.5423; my italics). Wittgenstein believed that this way of analyzing perceptual statements could account for so-called ambiguities of perception - as in the case of the Necker-cube - due to different "manners of seeing" as James called it (cf. Wittgenstein 1921a, 5.5423; also James 1890b, p. 253-257 and Mach 1910a, p. 92). Now this account is a special case of the general doctrine that "perception is a function of stimulation". Wittgenstein did adhere to the hypothesis of psychophysical parallelism; that differences in experience (as evidenced by judgments) correspond to differences in stimulation variables seems already to have been the drift of an example of an "internal relation", occurring in the notes dictated by Moore (cf. Wittgenstein 1961a, p. 85; p. 117).

Wittgenstein's account of thinking was already touched upon in the sketch of his "theory of judgment". A thought and a state of affairs of which it is a "picture" stand in an internal connection to one another, just as a sentence and a state of affairs do. A thought must therefore have constituents (which correspond to the words of language) - though Wittgenstein informed Russell that he did not know what those "psychical consituents" were (Wittgenstein 1974a, p. 72) [40].

Wittgenstein discussed the notion of volition primarily in connection with the problem of determinism and freedom of the will (cf. Wittgenstein 1961a, p. 43; Wittgenstein 1921a, 6.37 ff.). His doctrine that "there is only a logical necessity" had important consequences for his practical philosophy, as I have argued elsewhere (Visser 1981a, p.

404). But an elaborated note on 4.11.1916 contains an example which might be of interest for the psychological study of "the will as phenomenon" (cf. Wittgenstein 1921a, 6.423). It concerns the difficulty of drawing a square with the two diagonals in front of a mirror. This is a remarkable example of how one "starting from experiences of the physical world can penetrate into the field of psychology" (cf. Mach 1910a, p. 101-102). Wittgenstein's remark that one is only able to draw the figure "if one refrains completely from the visual picture and relies only on muscular feeling" (Wittgenstein 1961a, p. 87) seems to be compatible with the physiological view that all phenomena of volition are intelligible in terms of organic-physical forces (cf. Mach 1911a; 1919a, p. 140). In any case, Wittgenstein did not assume a kind of psychical causality: "Der Willensakt ist nicht die Ursache der Handlung, sondern die Handlung selbst". "Dass ich einen Vorgang will, besteht darin, dass ich den Vorgang mache, nicht darin, dass ich etwas Anders tue, was den Vorgang verursacht" (Wittgenstein 1961a, p. 87; p. 88).

I now turn to the subject of a priori propositions. Russell devoted a whole chapter of <u>The problems of philosophy</u> to the problem of how a priori knowledge is possible. When Wittgenstein wrote that he believed to have in principle solved all philosophical problems, he could not maintain this without taking a stand on this issue. As early as 1914, he announced an elucidation of Kant's question 'How is pure mathematics possible?' through the theory of tautologies. Eventually, the doctrine of saying and showing dictated Wittgenstein's solution of the problem: whereas every scientific proposition can be logically analyzed in an adequate symbolism, this is not possible for any of the a priori propositions discussed by Russell in <u>The problems of philosophy</u>. However, they are not absurd; their content is reflected in the "logical properties" of the symbolism. This is a different explanation of "a priori knowledge" than Russell's ("<u>All</u> a priori <u>knowledge</u> deals exclusively with the relations of universals": Russell 1913a, p. 162). But there are remarkable parallels between Russell's treatment and Wittgenstein's account, as to the <u>kinds</u> of statement claimed to have an a priori character. In the chapter "On our knowledge of general principles", Russell mentioned the propositions of logic and pure mathematics. The first include "principles which enable us to prove, from a given premiss, that there is a greater or least probability that something is true" (o.c. p. 114). Wittgenstein also considered some probabilistic propositions a priori and even tried to point out what the different sources were for these three kinds of "a priori truths". Generally speaking, he explained logical principles in a narrow sense in terms of truth-grounds (Wahrheitsgründe), principles of probability on the base of numbers of truth grounds, and arithmetical truths in terms of logical operations. They all have in common that "their correctness can be seen without our having to compare what they express with the facts as regards their correctness" (cf. Wittgenstein 1921a, 6.2321). Russell said about a priori knowledge that "we see its truth without requiring any proof from experience" (o.c., p. 116); he even went so far as to say "we feel that two and two would be four" in "any possible world": "this is not a mere fact", such as an empirical generalization, "but a necessity to which everything actual and possible must conform" (o.c., p. 121). We have seen that Wittgenstein also assumed a "connection" of logic with the world; "logic treats of every possibility" (Wittgenstein 1921a, 2.0121).

Russell saw a priori knowledge as knowledge about universals; his paradigm was the statement "two and two are four" (Russell 1912a, p. 169):

> Thus the statement "two and two are four" deals exclusively with universals, and therefore may be known by anybody who is acquainted with the universals concerned and can perceive the relation between them which the statement asserts. It must be taken as a fact, discovered by reflecting upon our knowledge, that we have the power of sometimes perceiving such relations between universals, and therefore of sometimes knowing general a priori propositions such as those of arithmetic and logic.

The notion of universal does not occur in Wittgenstein's earlier writings, but he has a kind of substitute in the formal properties and relations of objects and elementary situations, and in the internal properties and relations of "structures of facts". It is not surprising that Russell's example of two shades of green which have more resemblance to each other than either has to a shade of red (Russell 1913a, p. 160-161) returns in an example of two blue colours standing in the internal relation of brighter and darker. We have seen that it can only be shown that this internal relation holds, to be sure in the sentences about the colours in question within an adequate symbolism (cf. Wittgenstein 1921a, 4.122, 4.123). This is of great importance for Wittgenstein's attitude towards logical analysis. For it is clear that "ordinary language" does not "show" internal relations between colours. In this respect the physicalistic way of expression (again) fares much better (Wittgenstein 1961a, p. 81):

> Dass ein Punkt nicht zugleich rot und grün sein kann, muss dem ersten Anschein nach keine logische Unmöglichkeit sein. Aber schon die physikalische Ausdruckweise reduziert sie zu einer kinetischen Unmöglichkeit. Man sieht, zwischen Rot und Grün besteht eine Verschiedenheit der Struktur. Und nun ordnet sie die Physik gar noch in einer Reihe. Und nun sieht man, wie hier die wahre Struktur der Gegenstände ans Licht gebracht wird.

I conclude that Wittgenstein appealed to a result in the field of world descriptions, in order to establish the adequacy of a logical analysis, once again a sign that the distinction between logical analysis and ontological reconstruction had become blurred. Wittgenstein implicitly gave a special interpretation of some results of Russell's The principles of mathematics. In section 440 of this work, the impenetrability of colours was discussed together with the impenetrability of pieces of matter. Wittgenstein did the same in the rest of the above quotation (o.c.):

> Dass ein Teilchen nicht zu gleicher Zeit an zwei Orten sein kann, das sieht schon wieder aus wie eine logische Unmöglichkeit.
> Fragen wir z.B. warum, so taucht sofort der Gedanke auf: Nun, wir würden eben Teilchen, die sich an zwei Orten befänden, verschiedene nennen, und das scheint alles wieder aus der Struktur des Raumes und der Teilchen zu folgen.

That both impossibilities are due to a logical structure was explicitly stated in Wittgenstein's summary of the above quotations in the "Logisch-Philosophische Abhandlung" (Wittgenstein 1921a, 6.3751). But whereas Russell declared that, for example, Newton's laws of motion in no way can be taken as "a priori truths necessarily applicable to any possible material world" and that the a priori truths involved in Dynamics are only those of logic (Russell 1903a, par. 462), Wittgenstein argued that these are a priori intuitions of possible forms of the statements of science (Einsichten a priori über die mögliche Formgebung der Sätze der Wissenschaft). These are reflected in, for example, "the logical form" of a causal law, a law of least action, a law of conservation, etc.. Apparently, these logical forms determine part of the "logical structure" of the world.

What is also very special in this view is that, broadly speaking, mathematical physics would bring this logical structure to light. For this means that Hertz, for example, was partly right when he said that the first book of Die Prinzipien der Mechanik contains a priori propositions. But, in Wittgenstein's eyes, they cannot be considered a priori propositions in the sense of Kant, that is, resting upon "the laws of inner intuition" and logic; the assertion that logical laws are forms of thought and space and time forms of intuition must be interpreted differently: Hertz's a priori propositions have rather to be seen as laying bare the "logical roots" of mechanical principles in the sense of Mach ("Ueber das Prinzip der Erhaltung der Energie"; cf. Visser 1982a, p. 182-185); they present logical forms of world descriptions [40].

The above evaluation of Wittgenstein's philosophy supports the following conclusion: Wittgenstein can be seen as having a special attitude towards logical analysis and ontological reconstructionism: each adequate physicalistic ontological reconstruction embodies results of logical analysis. The right way of gaining logical results is this: describing the world in the way of mathematical physics; this will reveal logical connections and these are properties of "the logical structure of the world". They are shown in the forms and mutual "internal" connections of empirical statements.

It is possible that Wittgenstein believed that he had given a "synthesis" of what was correct in the works of Frege, Russell and Hertz. I consider it, however, confusion of logical analysis in the sense of Frege and Russell with ontological reconstruction in the sense of Hertz and Russell.

CHAPTER NINE

RUSSELL'S PHILOSOPHY OF LOGICAL ATOMISM

Russell's lectures on "the philosophy of logical atomism" present in many respects rather tentative answers to questions posed in the lectures on "our knowledge of the external world". This makes it difficult to draw definite conclusions from these answers. By way of introduction, I first draw attention to some remarkable features of Russell's ideas as they appear primarily in the *Introduction to mathematical philosophy*. Then I shall try to give a more systematic exposition by singling out three subjects; these will be treated in separate sections.

One of the problems which Russell faced in 1914 was the explication of "how logic and philosophy differ from the special sciences" (Russell 1914a, p. 208). Four years later his very general answer amounted to the following: *logic is a special "part" of a so-called logically perfect language*. It will be argued that this position induced a reorientation towards logical analysis in such a way that Russel could not consider it apart from ontological analysis or reconstruction. Only Ramsey saw through this muddle; his views will be discussed in the final section of this chapter.

The idea of seeing logic as a part of a logically perfect language was, of course, not new. We have seen that Wittgenstein introduced the notion of "a language which can express everything" in his notes to Moore, just in order to characterize logical propositions. (Moore showed these notes to Russell - cf. Wittgenstein 1974a, p. 59 - and this may explain how Russell found a characterization of logic that was not far removed from Wittgenstein's.)

The introduction of the notion of a logically perfect language provided both Wittgenstein and Russell with a method of philosophizing by means of statements *about* such a language. Only Russell went further by attributing to a <u>philosopher</u> of logical atomism the aim of constructing at least in principle a complete and logically perfect language, whereas Wittgenstein left the task of giving descriptions of the world to the natural scientist. For Wittgenstein, "the right method of philosophy would be: to say nothing except (...) propositions of natural science, i.e. something that has nothing to do with philosophy". (Although most statements of the "Logisch-Philosophische Abhandlung" are not in accordance with this method, they are nevertheless a means to reach the above conclusion.) The ontological reconstructionist Russell could not be satisfied with the scientific approach alone; since natural science assumed "unknown entities", the philosopher had the task of exhibiting such alleged entities as "logical constructions" of given entities and relations. How this could be done has been shown in Part Two.

How Russell philosophized by means of statements about an imagined or "ideal" language in which the world could be completely described, can easily be seen from the following quotation from the <u>Introduction to mathematical philosophy</u> (Russell 1919a; 1920a, p. 182):

> The first thing is to realize why classes cannot be regarded as part of the ultimate furniture of the world. It is difficult to explain precisely what one means by this statement, but one consequence which it implies may be used to elucidate its meaning. If we had a complete symbolic language, with a definition for everything definable, and an undefined symbol for everything indefinable, the undefined symbols in this language would represent symbolically what I mean by "the ultimate furniture of the world". I am maintaining that no symbols either for "class" in general or for particular classes would be included in this apparatus of undefined symbols.

Clearly the philosophical notion of "ultimate furniture of the world" is explained with the help of the notion of an ideal language. But this is not the only important feature of the quotation: it is also an extraordinary example of how logical and ontological issues are alluded to in one breath - I mean the logical problem of the (in)dispensability of the notion of class and the ontological problem about the (non)existence of classes of existents. The ideal language is seen to exhibit two features. On the one hand it reflects the (logical) claim that "class" does not have to be taken as a primitive idea of logic; for propositions in whose verbal or symbolic expression words or symbols apparently representing classes occur are analyzed in such a way that all mention of classes is eliminated. This is the well-known treatment of class-symbols as incomplete symbols, a result of logical analysis in the style of Russell's theory of descriptions. On the other hand, the ideal language shows that classes of existing individuals do not exist; because symbols for classes of such individuals are not among the undefined descriptive symbols. Thus a logical doctrine makes an ontological position possible by being used in the construction of the ideal language. We have here "a certain kind of logical doctrine, and on the basis of this a certain kind of metaphysics" (Russell 1918a; 1956a, p. 178). Wittgenstein would be satisfied: "philosophy consists of logic and metaphysics: logic is its basis" (Wittgenstein 1979a, p. 106; cf. Wittgenstein 1961a, p. 93).

We have seen earlier that the decisive argument for rejecting "class" as a logically primitive idea was a logical one. It appears that Russell continued to hold this view, witness the following discussion of the question whether classes can be regarded as "heaps" (Russell 1919a; 1920a, p. 183):

> We cannot take classes in the pure extensional way as simply heaps or conglomerations. If we were to attempt to do that, we should find it impossible to understand how there can be such a class as the null-class, which has no members at all and cannot be regarded as a "heap"; we should also find it very hard to understand how it comes about that a class which has only one member is not identical with that one member. I

> do not mean to assert, or to deny, that there are such entities as "heaps". As a mathematical logician, I am not called upon to have an opinion on this point. All that I am maintaining is that, if there are such things as heaps, we cannot identify them with the classes composed of their constituents.

Obviously Russell did not here confuse the notion of heap with "heaps" consisting of existing individuals. Moreover, he remained the agnostic logician who does not take a stand towards ontological problems. But there are indications that Russell lost his neutrality elsewhere in the Introduction to mathematical philosophy, notably in connection with what Ramsey called that paradigm of logical analysis, Russell's theory of descriptions.

In the chapter on "Descriptions", Russell criticized the doctrine attributed to Meinong that "the mountain", "the round square", etc. must have some kind of logical status. He argued that it suffered from a "failure of that feeling for reality which ought to be preserved even in the most abstract studies". As Russell explained in a notorious remark: "logic, I should maintain, must no more admit a unicorn than zoology can; for logic is concerned with the real world just as zoology, though with its more abstract and general features" (o.c., p. 169). This is an unexpected turn in a logical discussion; it might be explained by assuming that Russell was here implicitly referring to Montague's (1906a) alternative solution of the problem of denoting in The Journal of Philosophy, Psychology, and Scientific Methods. Montague had written that "between the universe of real existence and the universe of mere subsistence there intervene a number of systems, membership in which confers a relative existence". Russell, in turn, tried to make clear that there is only one "kind" of existence: "To say that unicorns have an existence in heraldry, or in literature, or in imagination, is a most pitiful and paltry evasion". "There is only one world, the "real" world: Shakespeare's imagination is part of it and the thoughts that he had in writing Hamlet are real". Logical analysis has to take this into account (o.c., p. 170):

> The sense of reality is vital in logic, and whoever juggles with it by pretending that Hamlet has another kind of reality is doing a disservice to thought. A robust sense of reality is very necessary in framing a correct analysis of propositions about unicorns, golden mountains, round squares, and other such pseudo-objects.

Saying, e.g., that there is a fact in an imaginative world that Hamlet was a Danish prince, is misusing the notion of fact. Logical analysis clearly presupposes a "realistic" notion of fact when it claims to be concerned with logical forms of facts (cf. the second lecture of Our knowledge of the external world). The lectures on "The philosophy of logical atomism" are explicit on this point. Again, Russell compared (philosophical) logic with zoology and said that it could be described as "a zoo containing all the different forms that facts may have" (Russell 1918a; 1956a, p. 216):

> In accordance with the sort of realistic bias that I should put into all study of metaphysics, I should always wish to be engaged in the investigation of some actual fact or set of facts, and it seems to me that that is so in logic as much as it is in zoology. In logic you are concerned with the forms of facts, with getting hold of the different sorts of facts, different logical sorts of facts, that there are in the world.

As a result, theory of knowledge and psychology are in some sense relevant to logic. It is true that this is so only in a very restrictive sense; a question such as whether or not belief is an isolated phenomenon, is "a subject belonging to psychology"; it is "only relevant to logic in this one way that it raises a doubt whether there are any facts having the logical form that I am speaking of" (o.c., p. 219). This seems serious enough, for it indicates a connection of logic with empirical studies: "you cannot really be sure that there are things having a given logical form, except by finding an example, and the finding of an example is itself empirical (...). Therefore in that way empirical facts are relevant to logic at certain points" (o.c.).

Russell's interpretation of the values of individual variables of a logcially perfect language as particulars, defined as terms of relations in atomic facts, fits well into this doctrine. But he made clear that "the question of what particulars you actually find in the real world" was "a purely empirical one which does not interest the logician as such". As a matter of fact, the logician is concerned only with logical propositions (o.c., p. 199):

> The logician as such never gives instances, because it is one of the tests of a logical proposition that you need not know anything whatsoever about the real world in order to understand it.

Here we have a point where Russell had no elaborated theory; several questions remained unanswered, such as which role logical forms play in logical propositions, let alone the problem what it is to "understand" a logical proposition. It is not enough to say that logical propositions themselves consist of symbols belonging to what Russell called "syntax" - the syntax of a logically perfect language - nor that such symbols merely express "forms of connexion". For this does not give a satisfactory condition for logical truths, as Russell himself realized (cf. o.c., p. 239-240).

Nevertheless, Russell did attempt to show that the connection of logic with ontology or metaphysics is closer than might be inferred from the above survey. This involved the following subjects: (1) logical analysis, (2) the semantics of the propositional calculus and quantification theory, and (3) the question of the logical form of belief-statements cq. "beliefs".

Logical analysis within the framework of an ideal language

The logical "language" of Principia mathematica by itself is not quite a proper tool for the logical analysis of sentences of ordinary language. In order to give a formal counterpart for such sentences, a certain so-called descriptive vocabulary has to be added. For example, the statement 'there are men' could be represented by a formula such as $\exists xMx$; this was the approach used by Russell in "On denoting" and in some illustrative examples of Principia mathematica. He realized that in some cases a straightforward analysis was impossible; an ordinary name, such as 'Socrates', had to be seen as an abbreviation for a definite description and an adequate logical analysis would have to take this into account.

This view of logical analysis did not wholly disappear in Russell's lectures on the philosophy of logical atomism (cf. Russell 1918a; 1956a, p. 234). However, the requirement that the logical language of Principia mathematica should be extended to a logically perfect language had a considerable impact on the very idea of logical analysis. Adding a vocabulary to this logical language now had to conform to the principle that in a logically perfect language "there will be one word and no more for every simple object, and everything that is not simple will be expressed by a combination of words" (o.c., p. 197).

The influence of the procedure of logical analysis can be seen from Russell's favourite example, the statement 'Piccadilly is a pleasant street'. Since 'Piccadilly' is the name for a certain portion of the earth surface, it is not the name of a simple object and therefore it can not be represented in the logically perfect language by a proper name. In order to find a formal counterpart of the word 'Piccadilly' in it, an ontological reconstruction along the lines of lectures 3 and 4 of Our knowledge of the external world would have to be carried out first. This means that Piccadilly would have to be characterized "as a series of material entities, namely those which, at varying times, occupy that portion of the earth's surface".

This view had repercussions even for the aforementioned account of definite descriptions in ordinary language (o.c., p. 200):

> The names that we commonly use, like 'Socrates', are really abbreviations for descriptions, not only that, but what they describe are not particulars but complicated systems of classes or series.

Logical analysis therefore becomes ontological reconstruction if it has to satisfy the requirements of a logically perfect language. This also holds for logical analysis in Russell's philosophy of mathematics; for just as ordinary objects were said not to be among the ultimate constituents of the world, numbers could not be either: "you do not have, as part of the ultimate constituents of your world, these queer entities that you are inclined to call numbers" (o.c., p. 290). Such is the outcome of transferring the logical view that a number has to be considered a class of classes of individuals to the context of an ideal language. For here the individuals in question can only be "individuals in the world", that is, existing particulars. This is a hard blow for logicism: the possibility of the logical analysis of arithmetic now depends on the existence of particular things.

It is well-known that, in his Introduction to mathematical philosophy, Russell tried to rescue a kind of limited logicism by distinguishing between the logical analysis of mathematical ideas and that of mathematical propositions. He maintained that logical analysis can show that all the ideas ("constants") that occur in pure mathematics are logical constants (Russell 1919a; 1920a, p. 202); the number-word 1, for example, could be defined with the help of propositions in such a way that "propositions in which 1 occurs acquire a meaning which is derived from a certain constant logical form" (o.c.). But he no longer subscribed to the view that all propositions of mathematics can be deduced from logical ones. The axiom of infinity which asserts that "there are infinitely many individuals" is an example. Its truth or falsity was seen to depend on the answer to the question of how many individuals there are in the world. The doctrine that logic is part of a logically perfect language in which the world can be completely described seems to have done its job. It even provided Russell with a kind of explanation of proper logical propositions; they are exactly those propositions which are true in every possible world, that is, they are independent of the number of individuals in the world (o.c.):

> We are left to empirical observation to determine whether there are as many as n individuals in the world. Among "possible" worlds, in the Leibnizian sense, there will be worlds having one, two, three, ... individuals. There does not even seem any logical necessity why there should be even one individual[1] - why, in fact, there should be any world at all.
>
> [1] The primitive propositions in Principia mathematica are such as to allow the inference that at least one individual exists. But I now view this as a defect in logical purity.

To be precise, $\exists x.x=x$ says that "at least one individual exists". Wittgenstein's interpretation of this formula as an empirical proposition was taken seriously.

It is remarkable that Russell failed to notice that the adequacy of the analysis of mathematical ideas depends on the analysis of mathematical propositions. For, as Russell himself argued in the third chapter of his Introduction to mathematical philosophy, if it is supposed "that the total number of individuals were (say) 10; then there would be no class of 11 individuals, and the number 11 would be the null-class. So would the number 12. Thus we should have 11=12". Clearly, in this case the logical analysis of arithmetical ideas would not be adequate.

There are indications that Russell's treatment was on the whole incoherent; in "The philosophy of logical atomism", he stated that existence-theorems in mathematics "establish that there is an object of such-and-such a sort, that object being, of course, in mathematics a logical object, not a particular, not a thing like a lion or a unicorn, but an object like a function or a number, something which plainly does not have the property of being in time at all" (Russell 1918a; 1956a, p. 256). Here again we have the pre-Wittgensteinian Russell. But in his treatment of the semantics of his logic, the influence of Wittgenstein is unmistakably present. This subject will be dealt with in the next section.

The semantics of the propositional calculus and the semantics of quantification theory

The doctrine that "logic is part of an ideal language" enabled Russell to formulate a "realistic" theory of truth and falsehood in the sense that the truth or falsehood of propositions depends wholly on whether certain facts obtain in the world. It can now be explained what "there are atomic facts" means: in a complete description of the world you would have to mention (atomic) facts that certain particulars have certain properties or stand in certain relations. Such atomic facts make the "atomic propositions" of a logically perfect language true or false. Part of this was already expressed in the second lecture of Our knowledge: "atomic facts are what determine whether atomic propositions are to be asserted or denied" (Russell 1914a, p. 53), but in "The philosophy of logical atomism" the extension to non-atomic propositions had become an important issue. This becomes immediately clear when we look at Russell's treatment of molecular propositions. In Our knowledge it was claimed that an assertion such as "If it rains, I shall bring my umbrella" is just as capable of truth or falsehood as the assertion of an atomic proposition, but that "it is obvious that either the corresponding fact, or the nature of the correspondence with fact, must be quite different form what it is in the case of an atomic proposition" (Russell 1914a, p. 54):

> It does not require for its truth that it should actually rain, or that I should actually bring my umbrella; even if the weather is cloudless, it may still be true that I should have brought my umbrella if the weather had been different.

But now compare the corresponding treatment in "The philosophy of logical atomism". The correspondence of a molecular proposition with facts is again said to be "of a different sort from the correspondence of an atomic proposition with a fact". But here it is a serious question whether "there are molecular facts", that is, whether or not such facts would have to be mentioned in a complete description of the world. The answer is no (Russell 1956a, p. 209):

> I do not suppose there is in the world a single disjunctive fact corresponding to '*p* or *q*'. It does not look plausible that in the actual objective world there are facts going about which you could describe as '*p* or *q*', but I would not lay too much stress on what strikes one as plausible: it is not a thing you can rely on altogether. For the present I do not think any difficulties will arise form the supposition that the truth or falsehood of this proposition '*p* or *q*' does not depend upon a single objective fact which is disjunctive but depends on the two facts one of which corresponds to *p* and the other to *q*: *p* will have a fact corresponding to it and *q* will have a fact corresponding to it.

This was also seen to explain why the so-called logical constants were not entities in a realistic sense. "You must not look about the real world for an object which you can call 'or', and say, 'Now, look at this. This is "or".' There is no such thing, and if you try to analyze '*p* or *q*' in that way you will get into trouble" (o.c., p. 209-210).

This doctrine is supported by the view that the "meaning" of logical connectives is entirely explained by truth-functional schemes, that is to say, all that is necessary to <u>know</u> the meaning of molecular propositions is "to know under what circumstances they are true", given the truth or falsehood of the atomic propositions from which they are made up. The formulation is due to Wittgenstein, who also eventually reached the conclusion that "there are no molecular facts" in the sense that such facts need not to be mentioned in a complete description of the world.

But though there are no molecular facts, it must be assumed that there are "negative facts" which make (some) "positive propositions" false (o.c., p. 214):

> A thing cannot be false except because of a fact, so that you find it extremely difficult to say what exactly happens when you make a positive assertion that is false unless you are going to admit negative facts.

That this is a difficult view appears from a question from the audience: "What is precisely your test as to whether you have got a positive or negative proposition before you?" (o.c., p. 255). Russell's answer is important for my evaluation of his "philosophy of logic": it again demonstrates the indispensability of the notion of a perfect logical language. Russell said that in such a language, "it would always be obvious at once whether a proposition was positive or negative". Moreover, he left no doubt as to the metaphysical character of the project (cf. o.c., p. 215).

It is remarkable that, in his "Logisch-Philosophische Abhandlung", Wittgenstein too succumbed to the metaphysical conclusion that there are positive and negative facts (cf. "Notes and logic"): "Das Bestehen und Nichtbestehen von Sachverhalten ist die Wirklichkeit. (Das Bestehen von Sachverhalten nennen wir auch eine positive, das Nichtbestehen eine negative Tatsache)" (Wittgenstein 1921a, 2.06; cf. Wittgenstein 1961a, p. 93-94 and Wittgenstein 1921a, 4.024; also 1961a, p. 94-95 and 1921a, 4.063). But Russell went further by assuming that "general facts" and "existence-facts", which make quantified propositions true, also belong to the "inventory of the world" (cf. Russell 1956a, p. 134-135). The admittance of general facts was defended with two arguments. The first is epistemological and was already formulated in Our knowledge of the external world. It amounts to the following: "if there is, as there seems to be, knowledge of general propositions, then there must be primitive knowledge of general propositions" (...). The second argument has an ontological undertone: "When you have enumerated all the atomic facts in the world, it is a further fact about the world that those are all the atomic facts there are about the world, and that it is just as much an objective fact about the world as any of them are" (o.c., p. 236).

In my view, the above account of Russell's outline of a theory of truth for the propositional calculus and quantification theory shows that he did not distinguish logic from the general ontology of the world. But there is also a connection between logic and specific ontological reconstructions in Russell's treatment of particulars. Though he emphasized that the following definition was a purely logical one, the definition at least presupposes the notion of atomic facts (o.c., p. 199):

Particulars = term of relations in atomic facts. Df.

Russell's point was that "the whole question of what particulars you can find in the real world is a purely empirical one which does not interest the logician as such". But this does not preclude that the individual variables of, say, Principia mathematica range over actual particulars. Russell might have thought that, as far as logical theory is concerned, everything was settled by the fact that (1) no names of particulars occur and (2) it is one of the tests (cf. o.c., p. 201) of a logical proposition that you need not know anything whatsoever about the real world in order to understand it (o.c., p. 199). But he could not have interpreted $\exists x.x=x$ as the empirical proposition 'There is at least one thing in the world' without the above assumption for individual variables. What is more, for Russell the only words which are used as names in the logical sense are words standing for particulars with which one is acquainted at a given moment (cf. o.c., p. 201). Such are the particulars which "you have to take account of in an inventory of the world" in the sense in which the ontological reconstructionist Russell tried to establish his philosophical world view in Our knowledge of the external world.

It is understandable that people in the audience found some difficulties in this view, all the more since Russell explained that a proper name "seldom means the same thing two moments running and does not mean the same thing to the speaker and the hearer" with the consequence that proper names are important "in the sense of logic, not of daily life". And indeed one of the questions was: "If the proper name of a thing, a 'this', varies from instant to instant, how is it possible to make any argument?" Russell's answer, which was not meant as a joke, shows how far logic had become estranged from the task of describing valid arguments within fragments of natural language (o.c., p. 203):

> You can keep 'this' going for about a minute or two. I made that dot and talked about it for some little time. I mean it varies often. If you argue quickly, you can get some little way before it is finished. I think things last for a finite time, a matter of some seconds or minutes or whatever it may happen to be.
>
> (Question: You do not think that air is acting on that and changing it? Mr. Russell: It does not matter about that if it does not alter its appearance enough for you to have a different sense-datum.)

I now turn to the question of the logical form of belief statements.

The logical form of a belief

In "The philosophy of logical atomism", Russell's analysis of propostions "with more than one verb" such as 'Othello believes that Desdemona loves Cassio' was anything but a definite theory. Nevertheless, it contains aspects which again show points of contact between logical and ontological questions - though this time not so much in the discussion of a kind of semantics as in references to reconstructionist approaches towards the phenomenon of belief.

Russell formulated "what constitutes the puzzle about the nature of belief" (o.c., p. 225) as the problem that in a proposition such as 'Othello believes that Desdemona loves Cassio' the verb 'loves' "seems to be relating two terms, but as a matter of fact does not" when the proposition 'Desdemona loves Cassio' happens to be false. He offered two elements of a (future) logical theory of belief statements; (1) their logical forms are principally different from the logical forms of atomic propositions, and (2) two belief statements of the form 'I believe p' and 'I believe q' have a different logical form if p and q are not of the same logical form. This is, of course, still far from a solution of the problem of the logical form of belief statements - as Russell himself realized (o.c., p. 227). But did he actually have any idea of what he was looking for? He didn't mention, as in the case of his theory of denoting of 1905, any logical puzzle which had to be solved by such a solution. That is to say, when he gave the example of George IV - who wished to know whether Scott was the author of Waverley but not whether Scott was Scott - it seems to have escaped his attention that this example offered a puzzle for a logical theory of belief statements. This example is also peculiar since its logical analysis had become affected by ontological reconstructionism. For Russell's view that 'Scott' was also an incomplete symbol - because in his ontological reconstruction Scott had to be considered a series of classes - blurs the distinction between names and descriptions. The conclusion that "the truth or falsehood of a proposition is sometimes changed when you substitute a name of an object for a description of the same object" collapses.

Direct involvement of ontological reconsiderations in the discussion of the logical form of a belief can be found in Russell's remarks on the theory of neutral monism. He realized that this theory, which assumed that the material constituting the mental is the same as that constituting the physical, was in fact a reconstructionist view with which he could sympathize. For it exemplified "Occam's razor" (cf. Russell 1956a, p. 221). But in the case of beliefs, Russell did not think that the neutral monists offered a reconstruction at all. He argued that they were simply denying that there was such a phenomenon as belief (cf. Russell 1956a, p. 219). At best they "explained away" beliefs, whereas Russell assumed that there are such facts (o.c., p. 222).

What is important about Russell's discussion is that it did not exclude the possibility of interpreting a reconstruction of a (scientific) theory of beliefs as a theory of the logical form of belief statements. One step further and there is in principle no reason why one could not speak of, say, "behaviouristic logic", as Russell did in his next paper "On propositions: what they are and how they mean". A behaviourist account of facts might then at the same time yield a theory of the logical form of these facts, along the same lines as Russell's own reconstructionist account of, say, Piccadilly as a series of classes of material entities. One would almost conclude that "logical analysis" <u>was</u> ontological reconstruction.

<u>Ramsey's reaction</u>

Is the combination of logical analysis and ontological analysis or reconstruction inevitable? Suppose that the answer is 'no', on the ground of the argument that ontological reconstructions are not involved in Frege's original contributions to logical theory. It might be replied that ontological issues <u>must</u> come into the picture as soon as "the foundations" of logical analysis are reflected upon. In other words, logical analysis and ontological reconstruction would be indissolubly connected from a "higher" point of view, at least in the sense that talking about "individuals" and "atomic facts" in the semantics for logical calculi cannot be seen apart from one's metaphysical views about "what there is". Is it possible to avoid such a conclusion? I shall argue that Ramsey succeeded in doing so when he re-examined the logical theory of Principia mathematica. Ramsey tried to characterize formal logic in such a way that it would have only "formulae for formal inference" without using extra-logical notions. He rid the notions of "individual" and "atomic propositions" of their dependence on an ideal language in which the world could be completely described.

I do not mean to assert that Ramsey always departed from the Wittgensteinian-Russellian conception of logical analysis; after all, he once wrote that the task of logical analysis is "not merely one of English grammar" (Ramsey 1925b; 1931a, p. 117):

> we are not school children analysing sentences into subject, extension of the subject, complement and so on, but are interested not so much in sentences themselves, as in what they mean, from which we hope to discover the logical nature of reality.

As a matter of fact, Ramsey was in some respects influenced by the way Russell and Wittgenstein formulated their problems. Examples are the question about the (in)completeness and (in)dependence of "objects" (o.c., p. 121-122) and the problem of the "logical analysis" of judgments. There is no doubt that Ramsey at certain times showed interest in metaphysical questions. However, the outcome of his discussions makes clear that he did not want to derive answers to such questions from logical theories: logic can take care of itself in the sense that it neither needs nor presupposes extra-logical or extra-mathematical ideas or notions. Eventually Ramsey seems to have banned all ontological considerations even from philosophy. In one of his last papers, he formulated a conception of philosophy in which the purpose of logical analysis and reconstruction was limited to piecemeal improvements in precision of meaning.

Ramsey's first important philosophical paper was a critical review of Wittgenstein's Tractatus logico-philosophicus, written shortly after the appearance of the first German-English edition of the "Logisch-Philosophische Abhandlung" (cf. Braithwaite 1931a, p. xii). He knew what he was talking about, for he assisted with the translation and in the preparation of the book for the press. That Ramsey read it carefully can be seen from his discussion of the notion of "form of representation" (Form der Abbildung). I shall not review this discussion, but want to draw attention to the fact that Ramsey tried to preserve Wittgenstein's "non-mystical" conclusions without reference to that "elusive entity, the form of representation, which is intrinsically impossible to discuss" (Ramsey 1923a, 1931a, p. 274).

A similar repugnance to a kind of metaphysics can be found in Ramsey's comment on Wittgenstein's so-called pseudo-propositions about "internal properties" of objects. He believed that it was possible "to give reasons why these sentences are nonsense and a general account of their origin and apparent significance, which have no mystical implications" (o.c., p. 281). He did not go so far as to explain how alleged "a priori propositions" about properties of space, time or colours could be interpreted as in some way dependent on the formal structure of the symbolism instead of on the structure of the world (cf. Ramsey, o.c., p. 282-283). For Ramsey found it hard to see how, for example, the following principle could be a "formal tautology": "if B is between A and D, and C between B and D, then C must be between A and D" - "considering between in point of time as regards my experiences". This kind of criticism seems to show that Ramsey wanted to eschew "questions of fact" within the scope of logic. We shall see that the point became a principal one when in a later paper Ramsey reconsidered the notion of atomic proposition.

Ramsey adopted Wittgenstein's notion of tautology. In his paper "The foundations of mathematics", he even called Wittgenstein's definition "one of the most important of his contributions to the subject". The question was only whether the propositions of symbolic logic and mathematics were tautologies in Wittgenstein's sense (cf. Ramsey 1925; 1931a, p. 5; p. 11). Ramsey hoped to be able to show that they were, in order to have an alternative to the formalist and intuitionist philosophies of mathematics. (He found the formalist theory inadequate, while (in his view) the intuitionist could not account for large parts of mathematics.)

The three obstacles to the logicist approach are the three hypotheses which Whitehead and Russell assumed: the axiom of reducibility, the axiom of choice, and the axiom of infinity. Ramsey formulated a simplified theory of types which made the first axiom superfluous. He did this by treating functions of functions in the same way as functions of individuals. The theory was given only in outline, but it appears that it could be made precise, and that Ramsey was right when he claimed that the axiom of reducibility could be dispensed with. (Cf. Hatcher 1982a.)

As for the axiom of choice, Ramsey sufficed with the remark that it became "the most evident tautology" as soon as one realizes that the class whose existence is asserted does not have to be definable by a propositional function of the sort which occurs in Principia mathematica: it has only to be required that the class in question is "defined" by a so-called function in extension to which every individual as argument associates a unique proposition as its value - the notion of "function in extension" being primitive in Ramsey's system. The result is that Ramsey got what he wanted, though his approach is not satisfactory from a technical point of view; he did not devise a formalization of such an axiom of choice, nor is his notion of tautology a clear one (cf. Leblanc 1976a, p. 293).

Ramsey also omitted a formalization of the axiom of infinity in his system. It had to be a different axiom than the axiom of the same name in Principia mathematica, since Ramsey rejected the definition of identity there used. His point, framed in a somewhat peculiar terminology, was that the general set-up of Principia mathematica was such that the three hypotheses were genuine or empirical propositions asserting "something about reality". With this, however, he did not mean to say that their truth depends on how the real world actually is, only that they are not true in all type hierarchies. As soon as it is logically possible that a proposition is true and that it is false, the proposition is neither a tautology, nor a contradiction.

Ramsey's comment on the axiom of reducibility in Russell's system explains how this has to be understood. He showed that it is an empirical proposition - "that is to say, neither a tautology, nor a contradiction" - as follows (Ramsey 1931a, p. 57):

(a) The axiom is not a contradiction, but may be true. For it is clearly possible that there should be an atomic function defining every class of individuals. In which case every function would be equivalent not merely to an elementary but to an atomic function.

(b) The axiom is not a tautology, but may be false. For it is clearly possible that there should be an infinity of atomic functions, and an individual a such that whichever atomic function we take there is another individual agreeing with a in respect of all the other functions, but not in respect of the function taken. Then $(\varphi).\varphi!x \equiv \varphi!a$ could not be equivalent to any elementary function of x.

A similar demonstration was given for the axiom of choice in the system of Principia mathematica. It has already been noted that Ramsey considered it "the most evident tautology" in his simple type theory: "I cannot see how this can be the subject of reasonable doubt, and I think it never would have been doubted unless it had been misinterpreted" (o.c., p. 58).

In Ramsey's system, however, the axiom of infinity asserts "that there are an infinite number of individuals". This *is* "a mere question of fact" (o.c., p. 59) and remains so when in the Wittgenstein-Ramsey semantics it is considered a *contradiction* in every "universe of discourse" with finitely many individuals, and a *tautology* otherwise. Ramsey explained this as follows (o.c., p. 61):

> It may be wondered how, if we can say nothing about it, we can envisage as distinct possibilities that the number of individuals in the world is so-and-so. We do this by imagining different universes of discourse, to which we may be confined, so that by 'all' we mean all in the universe of discourse; and then that such-and-such a universe contains so-and-so many individuals is a real possibility, and can be asserted in a genuine proposition. It is only when we take, not a limited universe of discourse, but the whole world, that nothing can be said about the number of individuals in it.

Ramsey's terminology is again misleading, but when he wrote about doing "logic for the whole world" he did not have in mind a (Wittgensteinian) conception of the world as "the totality of existent atomic facts". This can be seen from the last two paragraphs of "The foundations of mathematics", concerned with the cardinal number of the class of all individuals identical with themselves (o.c., p. 61):

> We can do logic not only for the whole world but also for such limited universes of discourse; if we take one containing individuals,
>
> $Nc'\hat{x}(x=x) \geqq n$ will be a tautology,
> $Nc'\hat{x}(x=x) \geqq n+1$ a contradiction.

> Hence $Nc'x(x=x) \geqq n+1$ cannot be deduced from the primitive propositions common to all universes, and therefore for a universe containing $n+1$ individuals must be taken as a primitive proposition. Similarly the Axiom of Infinity in the logic of the whole world, if it is a tautology, cannot be proved, but must be taken as a primitive proposition. And this is the course which we must adopt, unless we prefer the view that all analysis is self-contradictory and meaningless. We do not have to assume that any particular set of things, e.g. atoms, is infinite, but merely that there is some infinite type which we can take to be the type of individuals.

In this account, the axiom of infinity does not presuppose a realistic interpretation of the notion of individual any more. The mathematical concepts are no longer dependent on the availability of real objects of a kind, as in Russell's philosophy of logic. Consequently, when Skolem in his well-known paper "Ueber die Grundlagendiskussionen in der Mathematik" criticized Russell on this point, the criticism did not apply to Ramsey's analysis, in which the existence of an infinite series of numbers is not (based upon) an hypothesis about the real world.

But what about the following criticism, expressly directed against Ramsey's approach (Skolem 1929a, p. 12):

> (...) ich glaube aber kaum, dass viele seine Auffassung als eine Verbesserung ansehen werden. In der Tat sind die Betrachtungen RAMSEYS wesentlich darauf basiert, dass die Existenz der Mengen, Funktionen usw. von ihrer Definierbarkeit völlig unabhänging sein soll, und dass heisst wohl nach der Logistik unabhängig von allen logischen Konstruktionsmitteln überhaupt; die Mengen, Funktionen usw. existieren also sozusagen ungefähr wie die Tiere für einen Zoologen, und das scheint doch ein Zurückgehen auf ältere

naive Anschauungen zu sein.

Skolem's criticism seems to misjudge the purposes of a logical analysis of mathematics. It is not a contribution to the foundations of mathematics in the sense that Weierstrassian mathematical analysis has to be re-examined for the acceptability of the results. Apart from the necessity of a "reconstruction" in order to escape contradictions such as Russell's and Burali-Forti's, logical analysis takes mathematical results for granted. It does not impose requirements of decidability. Mathematical theories do not have to be rebuilt, as the intuitionists, following Kronecker, believed. Recall that even Frege's "constructive" logicism doesn't establish that a criterion of whether a given object falls under a given concept, must be effectively applicable. The logical analyst does not ask whether an underlying principle such as the axiom of choice or the axiom of infinity is "acceptable"; what matters is only what follows from such principles.

Ramsey's analysis did not go beyond the conclusion that it is impossible to put forward mathematical analysis except as a consequence of the axiom of infinity; it did not preclude that one might do "mathematics of a world with a given finite number of members" (cf. Ramsey 1926a; 1931a, p. 78-79); Ramsey remarked that "it might be interesting to try do develop a new mathematics without the axiom of infinity"; but clearly this is not a question of logical analysis.

In the foregoing pages I argued that Ramsey's reformulation of the system of Principia mathematica frees it from "ontological implications". But from this it does not yet follow that logical analysis of ordinary reasonings can do without ontology or has no points of contact with ontological reconstructions. One might still follow Russell in his philosophy of logical atomism.

Ramsey realized that Russell had been engaged in metaphysical questions such as the problem whether there is a fundamental division of objects into two classes, particulars and universals. He too saw that this problem could be treated in connection with logical reconstructions. But Ramsey's reaction was very remarkable. When one reads his paper "Universals", one can observe that he took the metaphysical problems seriously. The central question is whether to a distinction between two sorts of "logical constructions" (i.e. types), individuals and functions, there corresponds a distinction between two kinds of objects, particulars and universals. Ramsey implicitly answered that there does not. His point was that the logical difference between individuals and functions is simply due to the fact that "certain things do not interest the mathematician": "were it not for the mathematician's biassed interest he would invent a symbolism which was completely symmetrical as regards individuals and qualities" (Ramsey 1926; 1931a, p. 132).

Ramsey did not accept Russell's theory that each atomic fact must contain a term of a special kind, called a universal: "the truth is that we know and can know nothing whatever about the forms of atomic propositions" in Russell's sense (o.c., p. 133). That a logician can proceed without such knowledge indicates that his procedure is independent of the (Russellian) view that atomic propositions correspond to atomic facts. Now what is the procedure of the mathematical logician? As Ramsey described it (o.c., p. 133-134):

> He takes any type of objects whatever as the subject of his reasoning, and calls them individuals, meaning by that simply that he has chosen this type to reason about, though he might equally well have chosen any other type and called them individuals. The result of replacing names of these individuals in propositions by variables he then calls functions, irrespective of whether the constant part of the function is a name or an incomplete symbol, because this does not make any difference to the class which the function defines. The failure to make this distinction has led to these functional symbols, some of which are names and some incomplete, being treated all alike as names of incomplete

> objects or properties, and is responsible for that great muddle the theory of universals. Of all philosophers Wittgenstein alone has seen through this muddle and declared that about the forms of atomic propositions we can know nothing whatever.

The claim, attributed to Wittgenstein, that it is impossible to discover (Russellian) atomic propositions - which would correspond to "atomic facts" - by actual analysis is a philosophical claim. How would Ramsey be able to defend it while at the same time unwilling to be engaged in "mystical" considerations? Eventually Ramsey admitted that he had become doubtful as to the above claim. He seems to have recognized the possibility of establishing a philosophical distinction between individuals and universals in a formal ontological reconstruction (o.c., p. 137):

> If you think all or nearly all propositions about material objects are truth-functions of propositions about their location in events, then, on my view, you will regard material objects as adjectives of events.

But this still doesn't imply that Ramsey believed that formal ontological reconstructions influence the procedure of the logician. In his paper "Facts and propositions", he made clear what he means by "logical, mathematical, or formal inference or implication" (o.c., p. 151):

> The inference from '*p*' to '*q*' is formally guaranteed when 'If *p*, then *q*' is a tautology, or when the truth-possibilities with which '*p*' agrees are contained among those with which '*q*' agrees. When this happens, it is always possible to express '*p*' in the form '*q* and *r*', so that the conclusion '*q*' can be said to be already contained in the premiss.

Furthermore, Ramsey rejected in so many words the doctrine that logical analysis strives for the construction of an ideal language with names for every simple object in the world. Speaking about the supposition that an atomic sentence in a thinker's language "might after translation into a more refined language appear as nothing of the sort", he wrote the following lines against a contamination of logical analysis with ontological reconstruction (o.c., p. 152):

> If this were so it might happen that some of the combinations of truth and falsity of his atomic propositions were really self-contradictory. This has actually been supposed to be the case with 'blue' and 'red', and Leibniz and Wittgenstein have regarded 'This is both blue and red' as being self-contradictory, the contradiction being concealed by defective analysis. Whatever may be thought of this hypothesis, it seems to me that formal logic is not concerned with it, but presupposes that all the truth-possibilities of atomic sentences are really possible, or at least treats them as being so. No one could say that the inference form 'This is red' to 'This is not blue' was formally guaranteed like the syllogism.

Ramsey summarized this view in his thesis that the notion of an atomic proposition is relative to a language. I take this as implying that the application of logic does not have to wait for the construction of an ideal language reflecting one's ontological views, but can proceed without it.

From the foregoing analysis, the general conclusion is that logical analysis can be done in such a way that it does not commit us to ontological views. The assumption that it does, rests upon the (hidden) argument that the (formal) ontological reconstructions inherent in the approach to an ideal language are involved in logical analysis.

EPILOGUE

Ontological commitment

I have concentrated on a period covering about fifty years of analytic philosophy. Investigating developments of philosophical views on logic and logical analysis in the next fifty years (1930-1980) would require a new book. Yet I do not want to conclude this study leaving the last period wholly undiscussed: instead I shall examine a remarkable position held by one of the leading analytic philosophers, W.V.O. Quine. In his approach, an ostensibly Fregean position is given an unmistakably (Russellian) ontological turn.

Quine formulated the view at issue in his well-known Word and object. The following quotation expresses the gist of it (Quine 1960a, p. 161):

> (...) the simplification and clarification of logical theory to which a canonical logical notation contributes is not only algorithmic; it is also conceptual. Each reduction in the variety of constituent constructions needed in building the sentences of science is a simplification in the structure of the inclusive conceptual scheme of science. (...)
> The quest of a simplest, clearest overall pattern of canonical notation is not to be distinguished form a quest of ultimate categories, a limning of the most general traits of reality. Nor let it be retorted that such constructions are conventional affairs not dictated by reality; for may not the same be said of a physical theory? True, such is the nature of reality that one physical theory will get us around better than another; but similarly for canonical notations.

Recall that Frege also required of his logical notation that it be more than a "calculus ratiocinator": it has to be a "conceptual notation" (Begriffsschrift). But we see that Quine went further in the second part of the quoted passage, attributing to his canonical notation an undeniably ontological character. How did he arrive at such a view?

There is a general explanation that Quine's position was due to his scientism, characterized by the idea that each field of research is part of the whole of the (natural) sciences. According to Quine, the discipline called logic is also such a part, and therefore we might characterize his position nicely as "what is good for science is good for logic" (Bergmann 1955a, p. 256). Is logical analysis for Quine not "an endeavour within the framework of our scientific scheme of things"? (Cf. Orenstein 1977a, p. 153.)

However, I consider this explanation in itself not sufficient; it leaves unexplained <u>why</u> Quine's view that "the nature of reality" makes one physical theory better than another has an analogue in canonical notations. One of the claims of the foregoing chapters is that a program of logical analysis can very well be carried out independently of a program of formal ontological reconstructionism. So how is it to be understood that Quine attributed ontological implications to canonical notation in such a way that he eventually subscribed to Russell's view that "logic is concerned with the real world, though with its more abstract and general features"?

My answer to the last question is based upon a diagnosis of the role of Quine's so-called criterion of ontological commitment. As I see it, this criterion does not discriminate between products of logical analysis and formal ontological reconstructions. Purposes, originally belonging to the latter, could easily be transferred to the former. In this section, I look at some of Quine's early publications in order to justify this claim. I shall also pay attention to the so-called "nominalism", conceived by Quine in collaboration with Goodman. It will be argued that Goodman too in his book <u>The structure of appearance</u> failed to make a clear distinction between logical analysis and formal ontological reconstructionism. In particular his preoccupation with formal ontological reconstructions had such an impact on his notion of "individuals" that it lost its logical character.

Quine's interest in what he called ontological problems dates from the time that he presented his "Ontological remarks on the propositional calculus". In this paper he was concerned with "the entity, if any, whereof the sentence is a symbol" (Quine 1934a, p. 472):

> But what manner of things are these, whose names are sentences? Not facts, for that would leave no place for false propositions. Are they then judgments? Or abstract possibilities, Platonic ideas? Or are they merely, as with Frege, the two truth-values, truth and falsity?

It looks as if Quine is very close to a metaphysical discussion of the foundations of propositional logic in the spirit of the Russell of "The philosophy of logical atomism". But his answer to the above question contains no reference at all to such discussion. On the contrary, Quine made clear in the course of his argument that "in the theory of deduction, as a formal systematisation of certain aspects of the ordinary use of language and exercise of reason, there is no call to consider what manner of entity a proposition may be or to formulate the conditions under which propositions are identical". It is true that propositions, thus conceived, were considered "hypostatized entities, inferred denotations of given signs", but in a reference to his procedure in A system of logistic, he explains that when the story of deduction is woven into a broad and unified logistical system, "propositions" can be identified with "some manner of definite technical entities which figure also in other aspects of the total logistical system" (o.c., p. 473).

In later technical publications in logical theory, notably "New foundations for mathematical logic", one finds that Quine considers his alternative logical theories an improvement upon the theory of Principia mathematica because of their methodological advantages: less technical maneuvers, more deductive power (that is: the possibility of analyzing more reasonings in ordinary language) and fewer axioms. This means that Quine's discussion on this point - comparisons between different logical theories on the base of non-philosophical criteria of adequacy - stays within the limits of the Fregean program of logical analysis (cf. Part One).

However, Quine went another way in a paper presented on the Fifth International Congress for the Unity of Science (Cambridge Mass., 1939). In "A logistical approach to the ontological problem" he formulated what he called "a formal basis for distinguishing names from syncategorematic expressions" by describing names "simply as those constant expressions which replace variables and are replaced by variables according to the usual laws of quantification" (Quine 1939a; 1976a, p. 199):

> It is to names, in this sense, that the words 'There is such an entity as' may truthfully be prefixed. Elliptically stated: we may be said to countenance such and such an enitity if and only if we regard the range of our variables as including such an entity. To be is to be a value of a variable.

Here we have the logico-semantical notion of "entity" which Russell attributed to Frege in his "Substitutional theory of classes and relations". Only its use is not restricted to logical theory according to Quine. His so-called criterion of ontological commitment applies to all those "forms of language in which quantification figures as primitive and variables figure solely as adjuncts to quantification" (o.c., Quine 1976a, p. 199):

> What entities there are, from the point of view of a given language, depends on what positions are accessible to variables in that language. What are fictions, from the point of view of a given language, depends on what positions are accessible to variables definitionally rather than primitively.

The wide applicability of the criterion, however, contributes to a realistic interpretation of the notion of entity for logical, mathematical or semantic theories, when the following line of argument is accepted:

(1) The logical-semantical notion of entity is the same for logic, mathematics, formal semantics, as for axiomatizations of theories of natural science or common sense views, and their formal ontological reconstructions.

(2) The entities assumed in formal ontological reconstructions are to be considered the things that there are in the real world, in accordance with the point of view underlying those reconstructions.

(3) Differences between the disciplines mentioned in Premise 1 are in degree, not in kind; therefore the difference between the assumed entities in those accounts are in degree and not in kind.

Conclusion:

(4) The entities assumed in logical, mathematical or semantical theories can be seen as forming part of the real world.

Premise 1 is implicit in Quine's criterion of ontological commitment. Premise 2 follows from the very nature of the formal ontologist approach. That Quine was familiar with typical examples of formal ontological reconstructions can be seen from his early "Truth by convention", in which he explicitly mentioned Russell's reconstruction of instants of time (Quine 1936a; Quine 1949a, p. 268; cf. Quine 1976a, p. 100). The possibilty of different reconstructions of our knowledge of the external world, a phenomenalistic and a physical one, is acknowledged in "On what there is". Here we also read that one's "ontology" (as a part of metaphysics) is "basic to the conceptual scheme" by which one "interprets all experiences, even the most commonplace ones" (Quine 1948a, p. 29). This brings us nearer to the above conclusion, and indeed - given that the only way we can involve ourselves in "ontological" commitments is "our use of bound variables (Quine 1948a, p. 32) - Quine argues that one does not have to distinguish terminologically between existence, subsistence or being: the "entities" assumed in logic or mathematics are on the same footing as the entities assumed by the formal ontological reconstructionist. (Cf. Quine 1953a, footnote 1 to "On what there is".) And "even pure mathematics belong to the descriptive answer to the question what there is" was a final conclusion in the last section of Word and object.

The first part of Premise 3 is assumed in Quine's well-known theory of science. More interesting, from my point of view, is the second part of Premise 3.

That differences between entities, assumed in different "conceptual schemes" are only matters of degree, was stated explicitly in "Two dogmas of empiricism' (Quine 1951a; Quine 1961a, p. 45):

> Physical objects, small and large, are not the only posits. Forces are another example; and indeed we are told nowadays that the boundary between energy and matter is obsolete. Moreover, the abstract entities which are the substance of mathematics - ultimately classes and classes of classes and so on up - are another posit in the same spirit. Epistemologically these are myths on the same footing with physical objects and gods, neither better nor worse except for differences in the degree to which they expedite our dealings with sense experiences.

In other words (Quine 1951b, p. 71-72 - "Carnap's views on ontology" -):

> Within natural science there is a continuum of gradations, from the statements which report observations to those which

> reflect basic features say of quantum theory or the theory of relativity. The view which I end up with, in the paper last cited, is that statements of ontology or even of mathematics and logic form a continuation of this continuum, a continuation which is perhaps yet more remote from observation than are the central principles of quantum theory or relativity. The differences here are in my view differences only in degree and not in kind.

The above line of argument presents us with a general framework for an evaluation of Quine's "ontological" development. However, the actual course of events is more complicated than this simple argument might suggest, since originally, in "Steps towards a constructive nominalism", written together with Goodman, Quine held a traditional philosophical view, consisting of a "renunciation of so-called abstract entities". Their "nominalism" determined their preferences with respect to "any system", "any language" or "any theory" (Goodman and Quine 1947a, p. 105; p. 110, n. 10). This philosophical intuition already seems sufficient for reducing any two disciplines to the same denominator as soon as their commitment to kinds of "entities" is under consideration: Goodman and Quine cried wolf everywhere, as soon as they suspected the presence of "abstract entities". In this respect, the results of logical analysis in mathematics or ordinary language and formal ontological reconstructions of scientific theories are treated indiscriminately.

Unfortunately, Goodman and Quine did not make any attempt to explicate their view on "abstract enities" - a position which G. E. Moore once characterized as follows: "They seem to suppose that to call a thing an abstraction amounts to saying that it is hardly better than a pure fiction like a griffin or chimaera: something, therefore, which need not be reckoned as one of the constituents of the Universe" (Moore 1953a, p. 371-372). Since the "abstract entities" which Goodman repudiates comprise not only classes, but also relations, properties, functions, zoological species or whatever "universals", the position is not a question of logic alone - though logical considerations do play a role in their remark that "the most natural principle for abstracting (!, H.V.) classes or properties leads to paradoxes" (Goodman and Quine 1947a, p. 105). (This argument is given as an a posteriori consideration for fortifying their "philosophical intuition that cannot be justified by appeal to anything more ultimate".[42])

After their "Steps toward a constructive nominalism", both Goodman and Quine contributed to a further blurring of the distinction between logical analysis and formal ontological reconstruction. Nevertheless there are differences in their approaches. Goodman showed a preference for formal ontological reconstructionism, though he did foster special views on the role played by the logical apparatus, whereas Quine remained closer to Frege's program. In order to delineate Goodman's position more clearly, I shall first deal with his "nominalism" and then return to Quine's philosophy.

Goodman formulated a "nominalistic" program for the reformulation of any declarative statement in what he called the language of individuals. This is a formal language containing no names, variable or constant, for "entities" other than "individuals" - without restrictions on the predicates of these individuals. Goodman's idea was that each statement that can be analyzed in a Principia mathematica symbolism with class abstraction, could also be represented in a first order theory with proper axioms for a binary predicate, intuitively thought of as a part-whole relation. For in such a theory we can speak of the "sum" of all the individuals satisfying a certain predicate, that is, the individual which "overlaps" (i.e. has a common part with) all and only those individuals which overlap some individuals satisfying the predicate (cf. Goodman 1951a, p. 47) [43].

This calculus of individuals satisfied Goodman's philosophical starting point, though he admitted that it was inferior to higher-order predicate logic from a methodological point of view: less economy, less simplicity and a smaller scope. Yet this disadvantage was not considered a fundamental one, since he regarded the effort to carry out a "constructive nominalism" still too young for us to say where "the limits of translatability" might lie. Besides, he was especially interested in the reconstruction of so-called physicalistic and phenomenalistic "constructual systems" - I would say formal ontological reconstructions - where possible limitations of the calculus of individuals seemed negligible. (The calculus was explicitly said to be designed for finite sets of elements in the reconstruction of order of properties; o.c., p. 242.)

Goodman did not consider the calculus of classes, so frequently used by Russell and Carnap, a "purely neutral machinery that can be used without ontological implication in any constructional system" (o.c., p. 31). According to Goodman, when these two authors, following in the footsteps of Whitehead, thought that a reconstruction of, say, instants of time as classes of events was sufficient for not having to postulate such things as instants of time, they just let in more (kinds of) entities than is desirable (Goodman 1951a, p. 32):

> Thus when one uses and is unable to dispense with variables taking classes as values, one cannot disclaim the ontological commitment. Use of the calculus of classes, once we have admitted any individuals at all, opens the door to all classes, classes of classes, etc., of those individuals. Supposedly innocent machinery may in this way be responsible for more of the ontology than are the special frankly 'empirical' primitives.

Not only that, but Goodman believed that he could demonstrate how powerful his calculus was by presenting a general procedure for a formal ontological reconstruction of knowledge of so-called visual fields. And here it appears that a fundamental distinction between "individuals" in a logical sense and individuals as basic entities of a formal theory of the world cannot be made. Let me explain.

By assuming that the visual field of a perceiving subject consists of coloured patches, Goodman tried to show that he could represent knowledge about coloured patches in a (formal ontological) framework, the elements of such patches - times, visual-field places, and colours - being the basic individuals, called "qualia". For example, that there is a particular colour c_1 at a particular place p_1 at a certain time t_1 is described by the formula $\cancel{c}(c_1+p_1+t)$, expressing that the mereological sum of the elements c_1, p_1, and t_1 is a "concretum" in the sense that each two non-overlapping parts of it "occur with each other" or "are together", whereas the sum itself is not together with any individual. Here, the relation of "being together" (W) is a basic relation of the framework; it serves to distinguish "factually combined" individuals from "mere" sums. In general, qualia are introduced as certain ("atomic") members of the field of this fundamental relation W, namely those which have no members of the field of W as proper part.

This way of defining the "basic units" of a constructional system is similar to Whitehead's procedure of defining the fundamental entities of a formal ontological reconstruction, so that Goodman can be considered a formal ontological reconstructionist, only one who is working with an apparatus other than Whitehead's, Russell's or Carnap's (in Der logische Aufbau der Welt). But even if it is assumed that his preference for the calculus of individuals is based upon logical arguments - of the kind of the a posteriori considerations in "Steps toward a constructive nominalism", then it must still be admitted that Goodman failed to preserve a logical notion of individual. In the introductory comment on the presentation of the above-mentioned constructional system, he explained that this approach had the effect of restricting the range of the individual variables of the primitive predicate to the overlap relation of the calculus of individuals. He also argued that this restriction could be made superfluous with the help of appropriate adaptations (cf. o.c., p. 173-174). This implies, in my opinion, that Goodman's notion of individuals is so broad that it comprises not only individuals in a logical sense but also existing individuals. But then the general position called nominalism does not discriminate between logical analysis and formal ontological reconstructionism, and Goodman's reformulations of declarative statements always presuppose a particular ontology of the world. Only "nominalism" in a general sense is not yet the full-blown ontological position strived for by formal ontological reconstructionists. It "excludes", in Goodman's own words, "all except individuals but does not decide what individuals there are" (o.c., p. 149). A nominalist can still be a phenomenalist, or a physicalist. For example (idem):

> The nominalist who is also a phenomenalist will refuse to admit that there are any such individuals as electrons or magnetic fields or even things, if he cannot construe them as made up of phenomenal individuals. He will then have to construe the terms "thing", "electron", and "magnetic field" syncategorematically.

Goodman himself openly expressed a preference for a particular constructional system. Commenting on the choice of (phenomenalist) qualia as basic units, he mentioned some advantages of a reconstruction of knowledge of visual fields in terms of such qualia over Carnap's reconstruction in terms of Elementarerlebnisse. (Goodman's point of view was taken from Lewis' epistemological theory). This explains the fact that The structure of appearance is primarily considered a contribution to formal ontological reconstructionism and not to logical analysis. (Cf. Moulines 1973a.)

Quine did not follow Goodman's nominalistic program, that is to say, he realized - at least in due time - that "a thoroughgoing nominalist doctrine is too much to live up to" (Quine 1960a, p. 269). Instead of tying himself down to a particular instrument for analysis c.q. reconstruction such as Goodman's calculus of individuals, he required that a declarative statement be represented in a so-called canonical notation of logic in order to make "the ontological commitment" explicit. (A canonical notation can be described as a predicate-logical language form without free variables.) If possible, reformulations should be given in such a way that "entia non grata" are eliminated, since Quine kept on resisting "platonic" assumptions gratuitously.

In Word and object, it can be seen how indiscriminately Quine's criterion of ontological commitment works in seeking out "abstract objects" or "doubtful objects"; they are found in a variety of "conceptual schemes or frameworks", as the following list of such "objects" reveals: classes, attributes, propositions, numbers, relations, functions, points, degrees, miles, possible objects, and ideal objects. Moreover, his discussion whether such entities are "admissable" shows that results of logical analysis and formal ontological reconstructions are all alike when considered from an ontological point of view. Quine put talking about "propositions", "possibles", "concepts" in semantic analysis on a par with talking about our knowledge of the (external) world in terms of sense-data. His line of argument in the last chapter of Word and object, on "ontic decision", even suggests that the debate on the preference of physical objects - as "concrete" objects par excellence - over sense-data in an account of empirical knowledge is of the same order as a debate about preference of, say sentences over propositions in semantic analysis, at least in so far as the applicability of Quine's criterion of ontological commitment is at issue.

A most remarkable piece of evidence for this last implication can be found in the following quotation, in which Quine does accept what he called "a bifurcation in canonical notation" (o.c., p. 221):

> Which turning to take depends on which of the various purposes of a canonical notation happens to be motivating us at the time. If we are limning the true and ultimate structure of reality, the canonical scheme for us is the austere scheme that knows no quotation but direct quotation and no propositional attitude but only the physical constitution and behavior of organism. (...) If we are

> venturing to formulate the fundamental laws of a branch of science, however tentatively, this austere idiom is again likely to be the one that suits. But if our use of canonical notation is meant only to dissolve verbal perplexities or facilitate logical deductions, we are often well advised to tolerate the idioms of propositional attitude. Our purposes may then be well served by admitting the apparatus of propositional attitudes as of the end of 44 - hence minus the right to quantify over the attitudinal objects.

Eventually his view of the uniform applicability of the criterion of "ontological commitment", of the omnipresence of what was called semantic assent, led Quine to the standpoint that not only physics, but also pure mathematics and logic are part of the descriptive answer to the question what there is (cf. o.c., p. 275 and Quine 1970a, Ch. 7). I pointed out earlier that this sounds like an echo of Russell's comparison of logic with zoology in the Introduction to mathematical philosophy. But as if this is not yet enough, the author of the "Logisch-Philosophische Abhandlung" also did not speak in vain of "the logic of the world". For we see that Quine came very close to asking similar questions (Quine 1970a, p. 95):

> A logical truth, staying true as it does under all lexical substitutions, admittedly depends upon none of those features of the world that are reflected in lexical distinctions; but may it not depend on other features of the world, features that our language reflects in grammatical constructions rather than in lexicon? It would be pointless to protest that grammar varies from language to language, for so does lexicon. Perhaps the logical truths owe their truth to certain traits of reality which are reflected in one way by the grammar of our language, in another way by the grammar of another language, and in a third way by the combined grammar and lexicon of a third language.

It need hardly be argued that the influence of Quine's philosophy of ontological commitment is considerable - numerous contributors to philosophical logic have found it necessary to pay attention in some way or another to "ontological" presuppositions of their semantic theories. Some authors took things very seriously, such as David Lewis who, in his book Counterfactuals (1973), professed "realism about possible worlds" as a metaphysical doctrine supporting his formal semantics for counterfactual conditionals (Lewis 1973a, Ch. 4). More recently, Barwise and Perry have declared that their situation semantics for a systematic account of the logic of attitude words requires a realism not only about situations, objects, properties, relations, and locations, but also toward cognitive states and activities (Barwise and Perry 1981a, p. 691). Even a distinguished philosophical logician such as Hintikka - who did not show any metaphysical afterthoughts in his earlier work on free logic and doxastic logic - finds it necessary to defend his possible world semantics for "propositional attitudes" against Quine by formulating a kind of Kantian philosophical foundation for his semantic theories, as if this would help to support their adequacy.

In my view, the failure to make a distinction between logical analysis and ontological reconstruction is again responsible for that great muddle, the theory of ontological commitment in philosophical logic. That it is possible to do philosophical logic without ontological commitment through any use of language was demonstrated by Van Fraassen. If this is realized, ontological questions can be discussed in the place where they belong, without impeding the results of logical analysis.

NOTES

1. It is not quite clear whether Montague would have agreed with Van Fraassen. On the one hand, he seems to have thought that some sentences in everyday discourse "entail the existence of dubious epistemological, metaphysical, and ethical entities as pains, tasks, events, and obligations". On the other hand, he saw it as his task to "construct an exact and convenient language" for speaking of these entities, and to "analyze the pertinent notion of logical consequence" (Montague 1960a, p. 159-160). The result can, in concurrence with Thomason, be considered a purely mathematical theory which treats the "space of entities and possible worlds as bare, undifferentiated sets having no structure whatever" (Thomason 1974a, p. 50).

2. Cf. $\forall\varepsilon(\varepsilon>0 \rightarrow \exists\delta(\delta>0 \wedge \forall\gamma(-\delta\leqq\gamma\leqq\delta \rightarrow -\varepsilon\leqq\Phi(a+\gamma)-\Phi(a)\leqq\varepsilon)))$.

3. The sign for an assertible content, a horizontal stroke, has two functions; it indicates that the content which follows can be tested for correctness or incorrectness and is unified, so that other signs can be related to it (cf. Frege 1879a, p. 2 and Frege 1883a, p. 5); brackets are then unnecessary.

4. Cf. Zimmermann 1860a, p. 67. The preface of Zimmermann's Philosophische Propaedeutik is an important historical document in the development of the so-called anti-psychologism in logic.

5. Another writer who emphasized that there are differences between linguistic and logical forms was Lotze (see especially Lotze 1874a, p. 19-22). The question whether or to what extent Frege was influenced by Lotze is a matter of dispute. It is true that Frege did not break completely with the German logical tradition in Begriffsschrift: this is shown by his comment on a example of how ordinary language can be misleading in par. 9. In 'jede positive ganz Zahl ist als Summe von vier Quadratzahlen darstellbar' we do not have 'jede positive ganze Zahl' as the argument of the function 'als Summe von vier Quadratzahlen darstellbar zu sein': "Der Ausdruck 'jede positive ganze Zahl' giebt nicht wie 'die Zahl 20' für sich allein eine selbständige Vorstellung, sondern bekommt erst durch den Zusammenhang des Satzes einen Sinn" (Frege 1879a, p. 17). But it can be asked how seriously we must take this comment, since Frege also gave a positive account of the matter when he presented the judgment "du kannst als Argument für 'als Summe von vier Quadratzahlen darstellbar zu sein' eine beliebige positive ganze Zahl nehmen: der Satz bleibt immer richtig" as an example of how the argument can be indeterminate (unbestimmt). The formal representation of this judgment in "Anwendungen der Begriffsschrift" shows how great the distance was between Frege and Lotze. Though Lotze also used the notion "indeterminate" (unbestimmt) in a similar connection, he analyzed "einige Menschen sind schwarz' as 'einige Menschen, under denen jedoch nur die schwarzen Menschen zu verstehen sind, sind schwarze Menschen' (cf. Lotze 1874a, p. 80).

6. Cf.: 'it is false that $(\neg a \wedge c) \wedge (b \vee \neg c) \wedge (a \vee \neg b)$'.

7. The introduction of the material implication has presented difficulties to authors of introductions to logic even in our time. It is felt that the material implication has no exact counterpart in the German language, for example, in contrast with the conjunction. This is true in so far as "in natural language, no simple connection between the truth values of an implication and of its components can be established, because of the influence of the context in which these various sentences may appear" (Beth 1962a, p. 3). But if one abstracts from the pragmatic level as Frege did and restricts oneself to the possible consequences of given statements of a certain kind, then the Fregean connection can easily be established. One has to assume that certain principles underlie the reasonings which can be expressed in fragments of the German language with the binary connective 'wenn ... so ...'. The reader may consult Beth 1962a, p. 3-4.

Frege's explication of 

as "the circumstance that (the possibility that) A is denied and B is affirmed does not take place" is wider than an explication in terms of 'if' and 'then' for that matter; the formulation 'wenn ..., so ...' was reserved for the case that the judgment

can be made without knowing whether A and B are to be affirmed or denied (cf. Frege 1879a, p. 6).

8. If this means that theorems are easier proved with fewer axioms, then it is curious that Frege did not realize that this is also a matter of the complexity of the axioms (or axiom schemes).

9. Frege's famous "context principle" has been discussed in several places in the literature. According to Dummett (1973a; 1980a, p. 495) Frege (1884a) intended it to be understood as a defence of "contextual definitions". For didn't Frege's context principle shed light on the concept of the infinitesimal? (1884a, p. 71-71):

> Es kommt darauf an, den Sinn einer Gleichung wie df(x)=g(x)dx zu definiren, nicht aber darauf, eine von zwei verschiedenen Punkten begrenzte Strecke aufzuweisen, deren Länge dx wäre.

Here we have a differential equation in a form which is not at all strange in mathematics. We can take it for granted that Frege considered his context principle (that the words have a meaning only in the context of a proposition) valid not only for ordinary language, but *mutatis mutandis* also for ordinary mathematics. This is an obstacle when *logical conclusions* have to be drawn. In a conceptual notation - *Begriffsschrift* - every expression should have a meaning independent of the other parts of the proposition in which it occurs (1896a, p. 55-56). Nevertheless, one can in practice introduce abbreviations for complex expressions without bothering about any meaning of symbols which form part of the abbreviated symbol. This is clear from the abbreviations which Frege gave in the third part of Begriffsschrift (formula 69, 76, 99 and 115); their only purpose is "to bring about an external simplification". In the same way, one can abbreviate certain limit expressions with the help of differential expressions. This makes it possible to reintroduce expressions such as '$df(x) = g(x)dx$' in a rigorous build-up of mathematical analysis. But one does not need so-called contextual definitions in order to achieve this.

10. This task was announced as early as the Preface of Begriffsschrift, in which Frege promised to come to the fore immediately after this work with the elucidation - Beleuchtung - of the concepts of number, quantity, and so on. The reason why the execution of this plan appeared so late after this announcement was, at least according to the Preface of Grundgesetze der Arithmetik, partly due to internal transformations of the Begriffsschrift system (cf. Frege 1893a, p. IX).

11. Of course this proves nothing as long as it cannot be shown that Russell gave the right interpretation of Frege's philosophy. It could be that the situation is comparable with Russell's interpretation of Meinong's philosophy, which is considered incorrect by today's Meinong scholars; (cf. Lindenfeld 1980a, p. 202).

12. A consequence of these stipulations is that eventually $—4$ is the False and $-\!\!\top 2$ the True. $\vdash\!\!\top 2$ seems a curious judgment; however, it says only that 2 is not the True according to the theory of Grundgesetze.

13. Consequently,

$$\vdash\!\!\begin{array}{l}\top\!\!-\!\!-\, 3\\ \llcorner\!-\!-\, 2\end{array}$$

might be circumscribed as the assertion that 2 is not the True or 3 is the True.

The function

$$—\!\!\begin{array}{l}\top\!\!-\!\!-\, \xi\\ \llcorner\!-\!-\, \zeta\end{array}$$

is not very different from the analogue function in Begriffsschrift since one can regard $—\xi$ and $—\zeta$ as its arguments (cf. Frege 1891a, p. 28).

14. It can be argued that the question of an identity criterion for Sinne is only significant from the standpoint that Sinne form a kind of logical objects. The fact that Frege himself did not pose this question is something to think about. (Cf. note 16.)

15. Sometimes Frege used the expression 'Der Satz ... besagt, dass ...'.

16. The question whether two formulas of Frege's system of Grundgesetze have the same sense (Sinn) or not does not occur here.

17. Thoughts in "Ueber Sinn und Bedeutung" are put on the same level as commands and demands - as "senses" of imperatives and questions. I see no connection with the technical system of Grundgesetze.

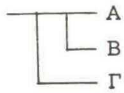
18. The same holds for many of his later "logical" investigations. A conspicuous example of sloppy thinking can be found in those fragments where Frege suggested an "identity criterion" for thoughts. I quote from the "Kurze Uebersicht meiner logischen Lehre" (1969a, p. 213): "Zwei Sätze A und B können nun in der Beziehung zueinander stehen, dass jeder, der den Inhalt von A als wahr anerkennt, auch den von B ohne weiteres als wahr anerkennen muss, und dass auch umgekehrt jeder, der den Inhalt von B anerkennt, auch den von A unmittelbar anerkennen muss (Aequipollenz), wobei vorausgesetzt wird, dass die Auffassung der Inhalte von A und B keine Schwierigkeit macht".

19. It was argued by Hinst, in a paper "Hätte Frege ohne Wertverlaufsfunktion auskommen können?", that numbers might be analyzed as second-level functions in Frege's first system: "Wenn also eine Zahlangabe eine Aussage von einem Begriff enthält, dann sind Anzahlen Funktionen 2. Stufe." (Hinst 1975a, p. 48). Hinst tried to show that this was possible by replacing Frege's definitions (Γ), (Δ), (Ε), (Ζ) and (Θ) of Grundgesetze I by definitions in which no recourse was made to value ranges or relations based upon value ranges. He pointed out - correctly in my eyes - that such was possible, since Frege had a method in Begriffsschrift for making statements about functions without representing them by value ranges.

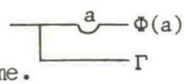
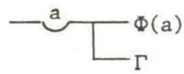
It is interesting to see how Hinst answered the question why Frege did not choose this way: one of the reasons could have been that the alternative method was syntactically more complicated than the method using value ranges (cf. Frege 1983a, p. IX). But Hinst argued that Frege's criteria - grammatical criteria according to Hinst - for what can be considered a function and what an object was decisive for analyzing numbers as objects. However, Hinst's own analysis is not really Fregean. Definition (I) of Frege has no analogue in Hinst's theory: Hinst has to define the number two with the help of the successor relation. But this means that Hinst not only has to define a number as having exactly one successor, but also to prove that this is so. Moreover, Frege had only one "type" of object and could treat relations between numbers exactly on the same footing as any other relation between objects; he had no problems with speaking about a number of numbers for instance. But Hinst has to give criteria for the identity for each type of function; in his analysis, numbers of numbers are not second-level functions.

20. Cf. $\hat{x}\exists G(x=\hat{y}Gy \wedge \neg Gx)$.

21. Frege's derivation is not completely transparent. The reader has to guess that the function $—\xi$ was taken for $f(\xi)$ in axiom (IIIa), taking in account so-called fusions (Verschmelzungen) of horizontal strokes. Frege also omitted an explication when he changed the order of components a formula of the form



He assumed that the reader was familiar with the fact that

 and 

are the same.

22. Cf. $\forall x(\neg x=\hat{y}Fy \wedge \neg x=\hat{z}Gz) \rightarrow (Fx \leftrightarrow Gx))$.

23. This statement has to be seen against the background of its time, in which an adequate theory of variables was lacking.

24. Cf. Russell's essay on "Recent work in the philosophy of mathematics"; here we find statements such as: "It has to be admitted that what a mathematician has to know to begin with is not much. There are at most a dozen notions out of which all the notions in all pure mathematics (including Geometry) are compounded".

25. The above account suggests that Russell's discussion of indefinables was not taken up until he had realized from his own situation that one could fail to grasp some primitive notions. Therefore he hastily inserted the sections 124, 125, 126, 127, 128 and 132, leaving the reader with the feeling that these sections are not directly related to the subject of Chapter IV, "addition of terms and addition of classes".

26. The subject is again dealt with in the discussion of Wittgenstein's views on logic and ontology in the "Logisch-Philosophische Abhandlung".

27. Russell's treatment, which is based upon Peano's work, was not very accurate. Pasch's theorem 11 or Huntington's postulate 5 are not derivable in the theory of the betweenness relation, given in par. 192 of The principles of mathematics. (A counterexample with four elements 1, 2, 3, 4 is {123, 143, 321, 341}.) In this respect the theory given in "Is position in time and space absolute or relative?" was better. However, here Huntington's postulate C is not derivable since a structure with exactly two elements 1 and 2 such that 2 is "between" 1 and 1 is allowed. It seems that Pasch's Vorlesungen über neuere Geometrie was not well read in the English-speaking countries. Even Huntington's "new" set of postulates of 1924 is not basically different from Pasch's second axiom system (cf. Pasch 1882a, p. 10-11).

28. According to Hertz, it is the task of mechanics "to derive occurrences and time-dependent properties of material systems from their time-independent properties" (Hertz 1894a, section 208). Material systems are defined in terms of particles and space. There is only one law - Grundgesetz - for so-called free material systems.

29. Cf. W. Mays 1961a, p. 236.

30. Cf. W. Mays 1961a, p. 235.

31. In each linear concept, the essential relation is a quinary relation in which four linear objective reals a, b, c, d and an instant of time t stand as soon as "the objective real a intersects the objective reals b, c, d in the order bcd at the instant t". Suppose now that two linear objective reals x and y are such that all linear objective reals u and v are such that when a given linear objective real a intersects x, u, v in any order at a given instant t then a intersects y, u, v in the order, and conversely. Then x and y can be said to have a position in the essential relation similar to each other with regard to a and t. With the help of this equivalence relation so-called intersection points or interpoints can be defined as classes of linear objective reals. Namely, a class of linear objective reals P is called an interpoint on a at t when an objective real x exists (which, together with two more objective reals, is intersected by a at t in some order) such that P is the class consisting of a and all linear objective reals with a position similar to x with regard to a and t. Next a class P of linear objective reals is called an interpoint of the essential relation R at the instant t, if a linear objective real a exists such that P is an interpoint on a at t. Finally the interpoints B, C, D are said to be in the interpoint-order BCD at the instant t with respect to the relation R when objective reals a, x, y, z exist such that (1) B, C, D are interpoints on a at t, (2) x, y, z are members of B, C, D respectively, and (3) a intersects x, y, z in the order xyz at t.

32. This theory could be used within a system with a quinary relation in the above sense thanks to a defined property - "homaloty" - of a class of linear objective reals which takes the place of a geometrical property of flatness.

33. Unfortunately, Russell's rough sketch of a reconstruction of "things" was the main subject - and target - of the principal reviews of Our knowledge of the external world. One wonders whether the criticisms would have been so strong if more attention had been paid to Russell's detailed treatment of the question of time. Only Hugo Bergmann, in his review in Kant-Studien, devoted a paragraph to Russell's reconstruction of "an instant of time", though he did not state the theory correctly. It is probable that Bergmann's review stimulated Carnap to read Russell's book; in any case Carnap continued the reconstructionist style and participated in "the creation of a school of men with scientific training and philosophical interest, unhampered by the traditions of the past, and not misled by the literary methods of those who copy the ancients in all except their merits".

34. In the article "On order in time", Russell considered the problem of arriving at a reconstruction of a one-dimensional continuum with the Dedekind-property. Here he faced the hypothesis that the class of events can be well-ordered. But he had already declared in the beginning of the article that this was an hypothesis, "which there is no reason to suppose true". This is remarkable, for he told a different story in The analysis of matter (Russell 1927a, p. 299-300):

> I have been led by the arguments, first of Dr. H. M. Sheffer, and then of Mr. F.P. Ramsey, to the view that Zermelo's axiom is true; I am therefore less reluctant than I should have been formerly to assume that events can be well ordered.

One wonders whether Russell wrote "On order in time" in 1935 as Marsh said (cf. Russell 1956a, p. 245); or did he present an old paper to the Cambridge Philosophical Society in 1936?

35. Russell omitted the italicised clause in assumption (b).

36. My proposal would be to distinguish two fields of research: (1) a logical treatment of verbs such as 'believes', 'knows' and 'perceives', and (2) an ontological and epistomological investigation op believing, knowing and perceiving. In (1), the notion of proposition might be used for a formal semantics of both perception and belief statements; in (2), the notion of fact might be used, by treating a perceptual situation as a relation between a perceiving subject and a fact. A belief situation would have to be treated differently.

37. At that time Russell did not discuss the position, formulated by Joachim, that "the true negative and the false affirmative judgment are 'about' the same real counterpart" (Joachim 1906a, p. 128: "Their difference consists in their contrary attitude towards the same reality" - with a reference to Aristotle's Posterior Analytics 89a, 23-37). As we shall see, it was Wittgenstein who brought "negative facts" back into the discussion. As to Russell, it seems that his argument from perception prevented him from speaking about negative facts (cf. his critique of Meinong). In that case it is all the more interesting that the above sketch of an alternative analysis of judgment influenced his logical theory, instead of the other way around, as in the case of the original position.

38. * 9.03 $(\forall x \varphi x \vee p) =: \forall x(\varphi x \vee p)$.

39. Wittgenstein's final treatment was slightly different; he seems to define, for a given function f, $\neg\exists x fx$ in such a way that it "said" that (it is the case) that every situation described by an elementary proposition of the form fx does not belong to the world.

40. It is interesting to see that Wittgenstein later - about 1930 - did not want to speak of an expectation, a thought or a desire that p "until these processes have the multiplicity which expresses itself in the proposition", in other words until they are articulated: "Gedanken nenne ich erst den artikulierten Vorgang; man könnte also sagen, 'erst das, was einen artikulierten Ausdruck hat'. (Die Speichelabsonderung im Mund - auch wenn sie noch so genau gemessen ist - ist nicht das, was ich Erwartung nenne.)" (Wittgenstein 1964a, p. 69-70). Eventually, he identified, in a certain case, "knowing" with "being able to describe" (Wittgenstein 1953a, 1958a, p. 185). It goes without saying that this does not necessarily mean: being able to describe in words (cf. o.c.). The later Wittgenstein considered the task of the psychologist: observing the external reactions (die Aeusserungen), the behaviour (das Benehmen) of a subject. That is, psychology was for him (still) one of

the natural sciences (cf. o.c., p. 151).

41. Of course, it is still a large step from this general diagnosis and the justification of particular structural properties of space, time and even colours. (Cf. Wittgenstein's answer to the question why a particle cannot be in two places at the same time.) As far as I can see, there is only one case in which Wittgenstein gave a complete argument why a certain principle must hold, namely his treatment of Russell's paradox: "Kein Satz kann etwas über sich selbst aussagen, weil das Satzzeichen nicht in sich selbst enthalten sein kann, (das ist die ganze "Theory of types")" (Wittgenstein 1921a, 3.332). It is possible that Wittgenstein considered this case as paradigmatic, and thought that all kinds of structural properties could be explained in a similar way, by an appeal to mathematical impossibility theorems.

42. Goodman and Quine even went so far as to declare: "Any system that countenances abstract entities we deem unsatisfactory as a final philosophy." This view determined their philosophy of mathematics in which they took the remarkable position that "sensory qualities afford no adequate basis for the unlimited universe of numbers, functions, and other classes claimed as values of the variables of classical mathematics". As if such qualities did afford an adequate basis for a limited universe of mathematical "entities". Apparently Goodman and Quine took Russell's discussion of the axiom of infinity in the Introduction to mathematical philosophy seriously - already a source of great confusion: does the nominalist have to "sacrifice" most of classical mathematics? It seems so, for the author of "A logistic approach to the ontological problem" wrote that the nominalist had only one resort: "he may undertake to show that those recalcitrant fragments are inessential to science" (Quine 1939a; 1967a, p. 202). What is good for science is good for mathematics? (This kind of confusion recurred when forty years later a philosopher formulated a similar way out for the "nominalist": "showing that there is an alternative formulation of science that does not require the use of any part of mathematics that refers to or quantifies over abstract entities" (Field 1980a, p. 2).)

43. From a technical point of view, Goodman's theory was just an elaboration of a so-called mereological theory, characterized by (1) axioms for a partial ordering of a part-whole relation, (2) a definition of an overlap relation, (3) axioms guaranteeing that each two individuals have an unique "product-individual", and (4) axioms guaranteeing that each two individuals have an unique "product-individual" if, and only if, they overlap each other.

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NAME INDEX

SUMMARY IN DUTCH

In het proefschrift worden de ontwikkelingen in de analytisch-filosofische traditie van Frege tot de vroege Wittgenstein beoordeeld aan de hand van een onderscheiding tussen logische analyses en formele ontologische reconstructies. Beargumenteerd wordt onder andere

(1) dat Frege als grondlegger van de methode van logische analyse geen ontologische overwegingen liet gelden bij de ontwikkeling van zijn logische theorieën (Begriffsschrift, Grundgesetze der Arithmetik),
(2) dat zulke overwegingen volgens Whitehead ("On mathematical concepts of the material world") in een ander kader (van formele ontologische reconstructie) thuis horen, en
(3) dat Russell ("The philosophy of logical atomism") en Wittgenstein ("Logisch-Philosophische Abhandlung") wel ontologische overwegingen gaven bij hun logische theorieën, onder andere omdat zij het bovengenoemde onderscheid uit het oog verloren.

In deel Een wordt Frege's methodologie aan een nadere beschouwing onderworpen. Beargumenteerd wordt dat Frege in zijn vergelijking tussen zijn logische theorie van Begriffsschrift en de Booleaanse logica een beroep deed op methodologische criteria. De stelling van Kluge dat Frege's onderscheidingen ontologische categorieën betreffen wordt afgewezen met het argument dat Frege's toelichtingen op zijn fundamentele logische onderscheidingen slechts een metaforisch karakter dragen. Speciale aandacht wordt besteed aan Frege's constructieve procedures om tot een logische theorie voor de rekenkunde te komen. Beargumenteerd wordt dat Frege's eis dat elke uit significante uitdrukkingen samengestelde zin een waarheidswaarde moet hebben een kwestie is van begripsprecisie. Ontologische interpretaties van problematische passages in Frege's artikel "Ueber Sinn und Bedeutung" worden afgewezen met behulp van een explicatie van Frege's opmerking over oordelen als "onderscheiden van delen binnen de waarheidswaarde" die nauw aansluit bij zijn syntaktische uiteenzettingen in Function und Begriff. Bell's stelling dat Frege "wetenschappelijke objectiviteit" veilig trachtte te stellen door zijn fundamentele categorieën ontologisch te funderen wordt als ongegrond afgewezen. Dat voor Frege ontologische overwegingen geen rol speelden in zijn logische theorieën wordt bevestigd gezien in zijn reactie op Russell's weerlegging van de logische theorie van Grundgesetze der Arithmetik.

In deel Twee worden bijdragen van Whitehead en Russell op het gebied van formeel ontologisch reconstructionisme aan een nadere beschouwing onderworpen. Whiteheads uitwerkingen worden beschouwd als een voortzetting van het programma van Hertz met andere middelen; Russell's pogingen om allerdaagse kennis te reconstrueren worden in verband gebracht met Moore's positie in diens colleges van 1910-1911. Beargumenteerd wordt dat Russell's analyses van de wiskunde in The principles of mathematics niet als formele ontologische reconstructies moeten worden beschouwd, hoewel het laatste deel van dit werk belangrijke aanknopingspunten bevat voor Whiteheads principiële aanpak in diens verhandeling "On mathematical concepts of the material world". Beklemtoond wordt dat door Whitehead niet alleen een precieze notie van formele ontologische reconstructie werd ontwikkeld, maar ook een formeel apparaat werd gepresenteerd waar ontologische reconstructies in kunnen worden uitgevoerd. Russell's reconstructie van de mathematisch-fysische tijdsstructuur wordt gepresenteerd als een voorbeeld bij uitstek van formeel ontologisch reconstructionisme. Aangetoond wordt dat Russell hierbij aandacht besteedde aan vijf problemen die bij deze discipline kunnen worden gesignaleerd, variërend van het vraagstuk wat de theoretische structuur is die gereconstrueerd dient te worden tot het probleem hoe de hypothesen van de bereikte reconstructie kunnen worden verdedigd. Over het gebruik van zogenaamde klassen in formele ontologische reconstructies wordt de stelling verdedigd dat de definitie van bijvoorbeeld tijdsmomenten als klassen van gebeurtenissen noch logische, noch ontologische "entiteiten" creëert.

In deel Drie wordt onderzocht hoe Russell en Wittgenstein er toe kwamen logische analyse te vermengen met ontologische overwegingen. Hochbergs diagnose dat Russell al in "On denoting" bij zijn beruchte voorbeeld van de eerste regel van Gray's elegie op zoek was naar een ontologische analyse wordt afgewezen aan de hand van een nadere beschouwing van Russell's argumentatie dat het onderscheid tussen twee verschillende soorten van "betekenis" onhoudbaar is. Daarentegen wordt aan Grattan-Guinness toegegeven dat Russell ontologische problemen meende te moeten signaleren in zijn beschouwingen over de noties van "individu" en "elementaire propositie" in de tijd dat hij aan Principia mathematica werkte. Aan de hand van een beschouwing over Wittgensteins "logische filosofie" in diens "Logisch-Philosophische Abhandlung" en een behandeling van enkele onderdelen van Russell's filosofie van het logische atomisme wordt betoogd dat zulke ontologische problemen niet van het toneel verdwenen. Toch wordt, met name aan de hand van Ramsey's reactie, de stelling gehandhaafd dat logische analyses wel degelijk zonder ontologische overwegingen kunnen worden uitgevoerd.

In de Epiloog wordt een poging ondernomen om te verklaren hoe Quine na een min of meer Ramseyaanse start uiteindelijk tot de positie kwam dat ook zuivere wiskunde en logica een bijdrage leveren aan een descriptief antwoord op de vraag "wat er is". Hierbij wordt gewezen op de centrale positie die Quine's criterium voor ontologische stellingname in zijn filosofie van de logica is gaan innemen - met alle gevolgen van dien voor de wijze waarop door sommigen tegenwoordig filosofische logica wordt bedreven.